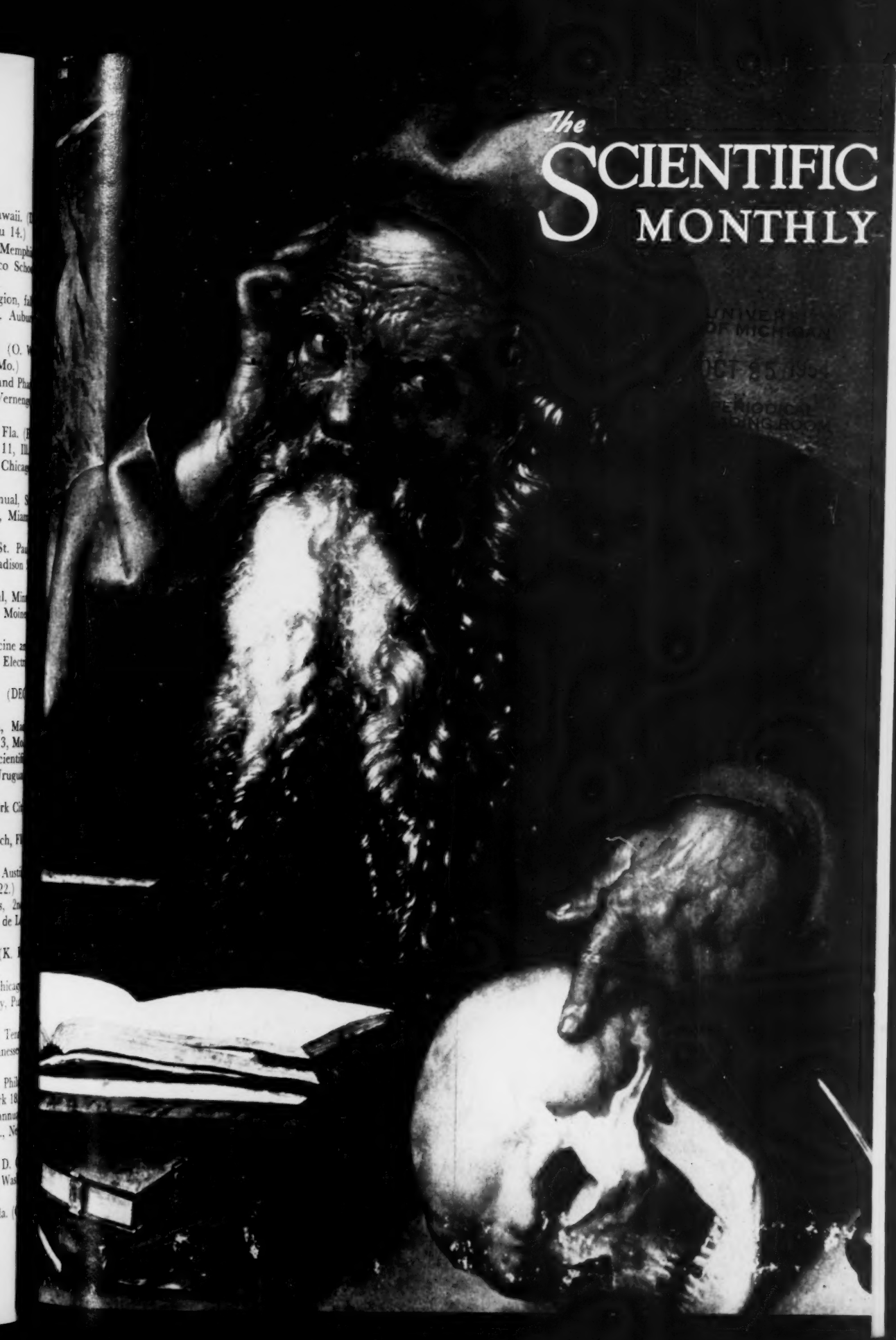
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Vacuum Coater with Built-in Photometer

A built-in optical photometer is used to measure coating thickness in a new vacuum coater. The photometer consists of a collimated light source and a sensitive photomultiplier detector. The detector can be placed to receive light reflected from or transmitted through the work. Thicknesses are measured by recording a change in reflected or transmitted light while the coating is being applied. Space is provided for placing any type of filter in the optical path, and the detector is sensitive enough to record full-scale the light reflected from plane uncoated glass. (American Instrument Co., Dept. SM, Silver Spring, Md.)

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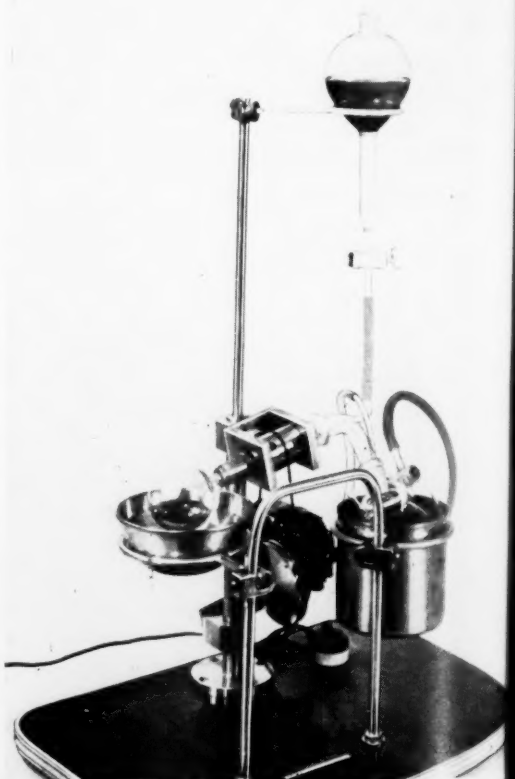

Fig. 1

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A creative, useful hobby is possible with a new copper enameling kit that comes complete with an electric kiln. The kiln operates on ordinary house current, heats in 4 min, and fires enamels in less than 3 min. Solder is used to hold the parts together. Handsome copper pins, key chains, cuff links, and so forth, can be made by the beginner. (Modern:American, Dept. SM, 2023 E. Jackson St., Bloomington, Ill.)

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Laboratory Glassware Catalog

A new catalog, "Custom-made glassware by Corning," lists more than 2600 items available for the construction of specially designed laboratory apparatus. Usually all items will be fabricated to the tabulated specifications, but slight variations may be indicated. Catalog CA-1. (Corning Glass Works, Dept. SM, Special Apparatus Section, Laboratory and Pharmaceutical Department, Corning, N.Y.)

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Lite-Tite darkroom ventilator is dome shaped and 12 in. in diameter. Mounted on plywood backing, it can be installed over a 4-in. wall exhaust opening with four screws. The fan is an 8-in. propeller type. (Stratfield Co., Dept. SM, 434 Grand St., Bridgeport, Conn.)

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A new mercury vapor rectifier tube, type 6508, has a directly heated oxide-coated cathode, a peak inverse voltage rating of 21 kv, and a voltage drop of 14 v. (Amperex Electronic Corp., Dept. SM, 230 Duff Ave., Hicksville, L.I., N.Y.)

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THE SCIENTIFIC MONTHLY

NOVEMBER 1954

Strange Life of the Dicyemid Mesozoans

BAYARD AND EVELYN McCONNAUGHEY

Dr. McConnaughey received his training at Pomona College, the University of Hawaii, and the University of California. Since 1948 he has been assistant professor of biology at the University of Oregon. From 1942 to 1946 he served as a U.S. Army medical laboratory technician in Hawaii, Okinawa, and Korea and in 1948 was a research associate at the Scripps Institution of Oceanography. Mrs. McConnaughey attended the College of Idaho and the University of Oregon and has collaborated on research on Florida Mesozoa at Oceanarium, Marineland, Florida.

ONE of the most singular groups of animals known to the zoologist is a small family of minute worms, the dicyemid mesozoans. In structure, life-cycle, and even their habitat, they strike one as strange, as something apart from the usual patterns of animal life.

The dicyemids are minute ciliated worms that live crowded together by the thousands, hanging in the kidney tissue of octopuses and squids, their bodies floating freely in the urine of their host. Here they live and reproduce, doing their host no apparent harm.

When examined microscopically they reveal a remarkably simple yet very unique structure (Fig. 1). The body consists of a single long internal cell, the axial cell, reaching almost the full length of the worm, surrounded by a single layer of large ciliated cells. The ciliated cells at the anterior end are modified to form a sort of head, called the calotte, by which the animals hang onto their host's kidney. There is no trace of any differentiated digestive, nervous, respiratory, or excretory system; no muscles or sense organs; in fact no organs, tissues, or glands in the usual sense of the words.

About 35 species of dicyemids are known. The size of the adult worm and the number of cells composing its body are characteristic for each species. Adults range from less than ½ mm in length in small species to as much as 10 mm or more in large ones. Each kind of octopus or squid in coastal areas is usually parasitized by a particular species or group of species of mesozoans. In many areas it appears that every octopus from early infancy is heavily infested with dicyemids.

Since the discovery of mesozoans by Krohn in 1839 almost every zoologist who has studied this group has interpreted differently their life-cycle and relationship to other animals. E. van Beneden, the first to study them by modern methods, was so impressed with their structural simplicity that he concluded they represent a distinct branch of the animal kingdom, intermediate between the protozoans, or one-celled animals, and the metazoans or many-celled animals. Thus he coined the name Mesozoa

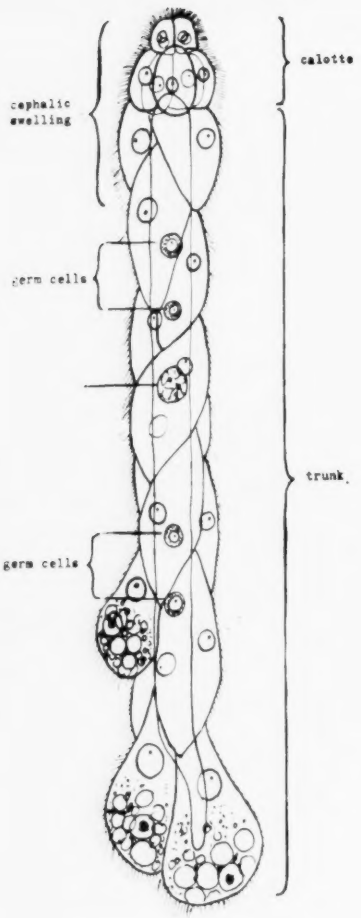


Fig. 1. Young dicyemid greatly enlarged.

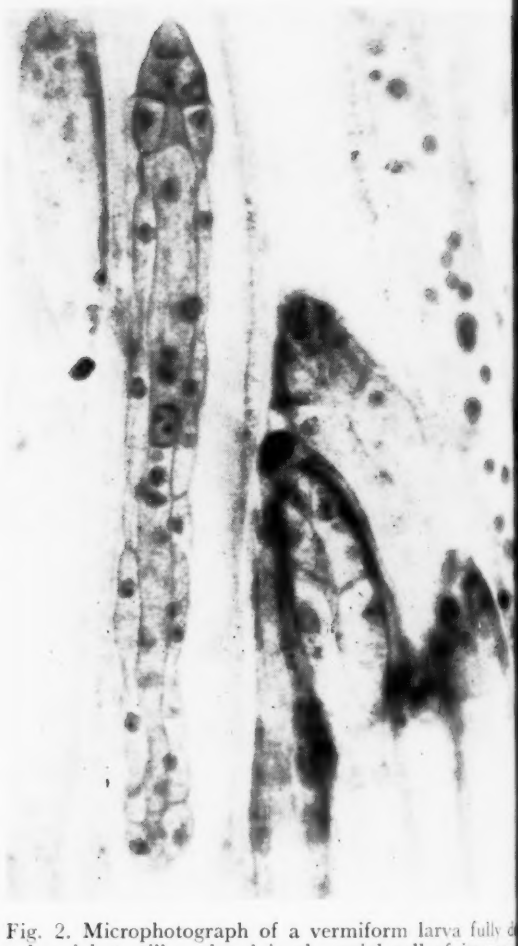


Fig. 2. Microphotograph of a vermiform larva fully d
veloped but still enclosed in the axial cell of its pare
nematogen. Portions of other dicyemids also show in th
picture. Note the axial nucleus and four germ cells alread
present in the axial cell of the larva.

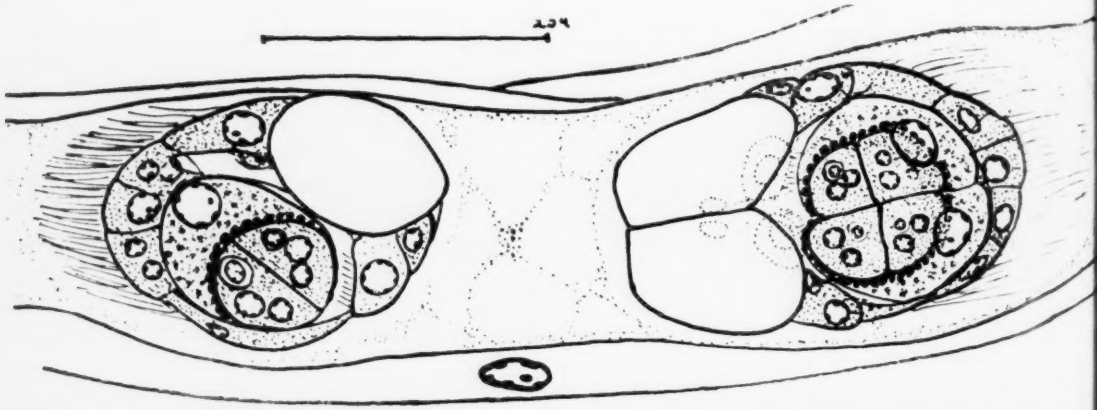


Fig. 3. Fully developed infusoriforms in the axial cell of a rhombogen, parasagittal and frontal views.

describe the group. Most subsequent investigators do not regard them as true intermediates but as descendants of some primitive metazoan stock degraded through parasitism.

Adult mesozoans bear two entirely different sorts of offspring. In one octopus they may all be giving rise to wormlike larvae similar to themselves (Fig. 2), while in another octopus they may be liberating peculiar short larvae, looking for all the world like infusorians or ciliated protozoans (Fig. 3). The first type of adult, the one bearing larvae like itself, is called a nematogen, and its offspring are called vermiform (wormlike) larvae. The second type of adult has been termed a rhombogen, and its peculiar offspring are known as infusoriform (infusorian-like) larvae. Young octopuses usually harbor the nematogen phase, whereas older ones harbor rhombogens or a mixture of nematogens and rhombogens.

In both phases the development of the larvae takes place intracelluluarly—that is, within one cell, the axial cell, of the parent organism. In this respect development differs markedly from that of all other animals. Before being born, the larvae attain their full complement of body cells, become ciliated and able to swim about, and have within them the germ cells that will give rise to the next generation. Birth is very simple. The larvae just push out or are squeezed out through the sides or ends of their parents, at almost any point. The exit closes immediately, leaving no scar, and the parent continues to develop and bear more larvae in the same manner. Depending upon the size of the species, the number of larvae developing at one time within the axial cell of an adult may range from one or two to 100 or more.

Once free, the vermiform larvae attach their heads to the kidney tissue of the octopus in which they are born and begin to grow in length. Growth consists simply in elongation and enlargement of existing cells, there being no more cell multiplication except for their contained germ cells. When mature, they begin to have offspring of their own. Thus the kidney soon becomes thickly populated with a vast number of parasites.

The infusoriform larvae, on the other hand, appear to be fully developed when they are liberated from the rhombogens. They immediately leave the octopus and become free-swimming in the sea water, presumably to find an intermediate host and there to give rise to the form that infects young octopuses.

It is not definitely known how young cephalopods are infected. They are usually infected shortly after hatching, presumably by some free-swimming agent that actively seeks them out and penetrates, perhaps by way of the urinary ducts, to the kidney. The stem nematogen, discovered by Lameere in 1916, is the earliest stage to have been found in baby cephalopods and is thought to be the infecting agent. Stem nematogens differ from ordinary nematogens in several respects, the most striking of which is the presence of two or three axial cells instead of one (Fig. 4). The larvae that they produce develop into ordinary nematogens with only one axial cell. These continue to reproduce as already described, populating their host with myriads of parasites.

About the time the host becomes sexually mature and its kidney heavily infested with mesozoans, a change of phase occurs in most of the nematogens. These changed individuals, now termed rhombogens, cease giving rise to vermiform larvae. In each rhombogen a few germ cells develop instead into peculiar reduced individuals, called infusorigens (Fig. 5), which never leave the axial cell of the parent rhombogen. The infusorigen in turn produces germ cells of a different sort, which it liberates into the axial cell of its parent rhombogen. Here they develop into infusoriforms. When mature, the infusoriforms leave the rhombogen and the octopus.

A rhombogen, liberating infusoriforms, has, then, the remarkable distinction of nurturing and giving birth to its own grandchildren.

The factor that induces the change of phase from

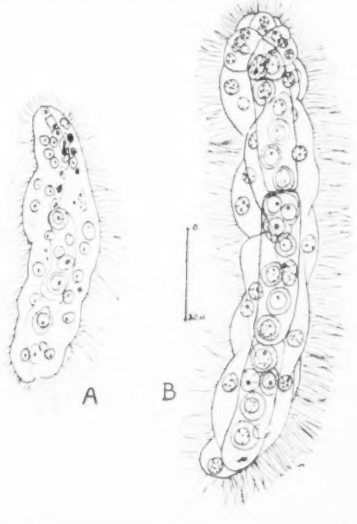


Fig. 4. (*A*) Larval stem nematogen of *Dicyemennea eledones* (an exceptional species in which the stem nematogen has two, instead of three, axial cells), showing the two axial cells and a row of abortive cells anterior to these. (*B*) Somewhat older larval stem nematogen of *Dicyema schulzianum*, showing the three axial cells, each with several enclosed germ cells. (*After Nouvel*).

nematogen to rhombogen is still unknown. Since it affects most of the mesozoans in an octopus almost simultaneously, it seems to be of the nature of an environmental change acting on the population as a whole rather than a maturing of individual parasites. Perhaps hormonal changes in the host incident to the attainment of sexual maturity have something to do with it, or it may be induced by the parasites themselves through limitation of available nutrients as the population grows, or by the liberation of certain metabolic products.

Occasionally an octopus or squid is found in which the dicyemids are in process of transformation. In these one can sometimes find individuals in which both kinds of larvae are being produced simultaneously (Fig. 6).

Several questions may be in the reader's mind at this point. Where do the germ cells come from and how do they get inside the axial cell? Are the processes involved in the formation of each kind of larva sexual, or is the entire known part of the life-cycle asexual, like the intramolluscan stages of trematodes? What is the nature of the axial cell—is it comparable to anything found in other animals or something wholly unique? These questions have puzzled many investigators. They can be answered only through close study of the developmental stages.

The dicyemids are ideal subjects for the study of embryology from fixed preparations. Use of the simple smear technique enables one to make permanent mounts without recourse to the involved histologic procedures of imbedding and sectioning. Several hundred individuals are often present on a single slide. A given individual may contain in it all the important embryologic stages simultaneously. In rhombogens these are even lined up in a row in proper sequence. The bodies of the dicyemids are small and transparent, so that in well-stained preparations every cell in the body of both the adult and its contained embryos and larvae can be carefully examined and clearly seen with the highest powers of the microscope.

Nematogen Phase

When a germ cell in a nematogen starts to form a vermiform embryo it divides unequally, the two daughter cells remaining broadly attached. The larger of the two cells remains quiescent while the smaller cell and its daughter cells continue to divide rapidly, forming a layer of small cells that envelops the larger one (Fig. 7). The embryo now begins to elongate, and differentiation of the head and trunk regions becomes evident. The large central cell now becomes active once more and undergoes a single division, again unequal. There are now two internal cells, a larger one anterior in positio which becomes the axial cell of the embryo, and smaller posterior cell, which promptly invades t axial cell to become the first germ cell. Additio germ cells are derived from this first one throu regular mitotic divisions.

In dicyemids of the genus *Dicyemennea* the em bryology is slightly more complicated (Fig. 8). I these, the large internal cell of the embryo divid twice instead of once, forming a row of three i ternal cells in the embryo. The anterior cell of t three is a small cell having the characteristics of a abortive axial cell and is gradually absorbed, lea ing no trace in the fully developed organism. T large middle cell becomes the axial cell of the lar and the posterior cell invades the axial cell to b come the first germ cell, as described in the prece ing paragraph.

Thus we see that the large central cell of ve young embryos is essentially reproductive in natu By divisions it gives rise to the functional germ ce and to the large axial cell that serves as a sort follicular cell, the function of which is to nour and protect the germ cells and developing embry The big departure from other animals lies in t invasion of this sterile follicular cell by the fun tional germ cell and the subsequent intracellul development of the germ cells and embryos. T production of an abortive second axial cell in me bers of the genus *Dicyemennea* may be regard as a sort of recapitulation or throwback to a p sumed primitive condition of the ancestors of d cyemids that had, perhaps, a row of internal repr ductive cells.

In this connection, Lameere's surprising disco ery that in some species the first nematogens to b found in very immature cephalopods have thr functional axial cells, instead of one, is of the gre est interest. These stem nematogens give rise larvae that are perfectly normal nematogens w

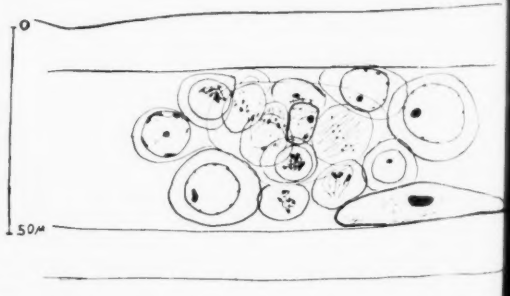
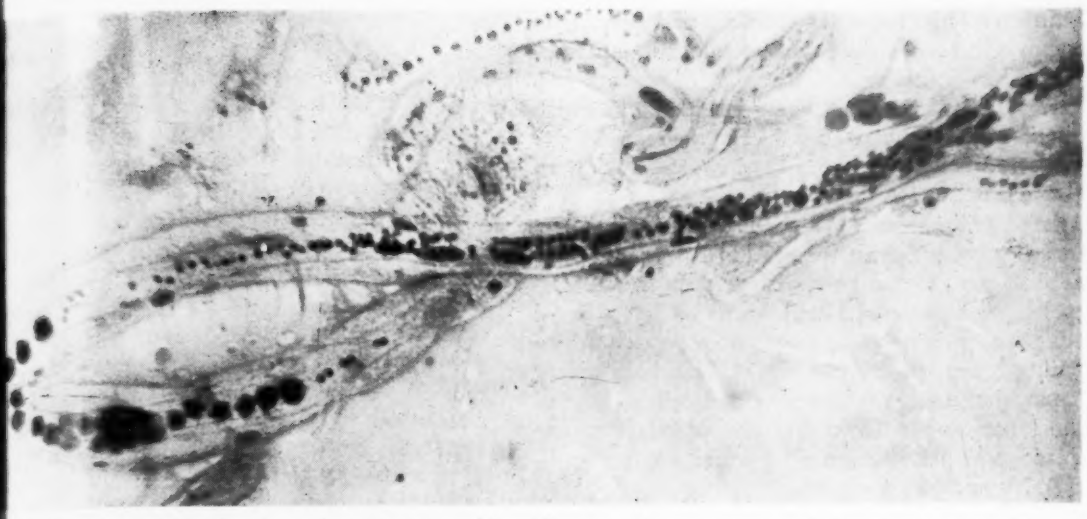


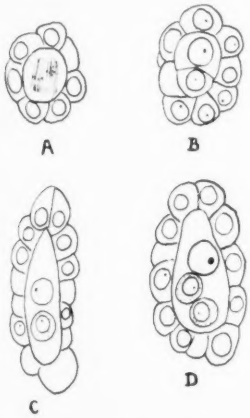
Fig. 5. Young infusorigen at the onset of reduct (meiotic) divisions. Clumping of chromatin at one side the nucleus is characterstic of cells about to unde meiosis.



6. Microphotograph of a large individual transitional between the nematogen and rhombogen phases. In most the trunk, the axial cell is still filled with ordinary germ cells and developing vermiform larvae (center and t, while posteriorly there is a fully developed infusorigen with a row of developing infusoriform embryos on h side (far left).

y one axial cell. Furthermore, Nouvel discovered 1935 that the very youngest stem nematogens to found, themselves still in larval condition, show ditional abortive cells anterior to the three func- nal axial cells. These abortive cells disappear as larva matures, being engulfed and digested by acent somatic cells in the same manner that the rtive second axial cell is in larvae of *Dicyemen-* Figs. 4 and 8).

These discoveries lend support to the theory that ancestral form from which dicyemids arose was acoelous, ciliated, wormlike organism with a of internal reproductive cells. This structure of the mesozoans and of their proposed ancestral form is in line with speculation by Hyman that perhaps one of the earliest steps in the evolution of the metazoans was the formation of a solid organism composed of peripheral somatic cells and enclosed reproductive cells rather than the formation of a hollow gastrula-like organisms as postulated by Haeckel.



7. Embryology of the vermiform larva in genus *ema*. (*A*) Young vermiform embryo, showing division e axial cell. (*B*) Resulting embryo with axial cell and germ cell. (*C*) Older embryo with first germ cell en- d in the axial cell. (*D*) Older embryo after division e first germ cell.

Rhombogen Phase

We have seen that the vermiform larvae arise asexually, directly from germ cells present in the nematogens. The events leading to the formation of infusoriform larvae are, in contrast, highly complicated, indirect, and sexual.

At the onset of the change of phase from nematogen to rhombogen, most of the germ cells present in the axial cell degenerate and either migrate into nearby epithelial cells, where they are digested, or disintegrate in the axial cell. A few lucky ones escape this fate. Each of these gives rise to a peculiar reduced sort of individual, the infusorigen, which is little more than a mass of reproductive cells living its entire life in the axial cell of the parent rhombogen (Figs. 5 and 9).

The infusorigen has the same general structure and embryologic development as a vermiform larva in its early stages but fails to differentiate into head and trunk regions. The fate of most of the cells comprising an infusorigen is very different from that of the corresponding cells in a vermiform larva. The peripheral cells, instead of becoming ciliated, round up, become very large, and develop into egg cells. The internal germ cells, after multiplying for

a time, undergo reduction divisions and give rise to sperm cells that fertilize the egg cells at the periphery of the infusorigen. Thus the infusorigen is a hermaphroditic, self-fertilizing individual in which all the cells except its axial cell are reproductive cells, and it lives its entire life inside the axial cell of the rhombogen.

In other sexual animals, the sexual stage that produces eggs and sperm is considered the mature, or adult, stage. Other stages, if present in the life-cycle, are considered larval. Here we have sexual adulthood strangely perverted and represented by a reduced, but sexually developed, individual living a parasitic existence in one of the cells of one of the larval stages. In having the sexual stages reduced and parasitic on an asexual stage, the dicyemids remind one of the situation in higher plants with their reduced gametophytes parasitic on the large sporophytes.

When fertilized, the egg cells detach themselves from the parent infusorigen and lie free in the axial cell of the rhombogen. There, following the usual pattern, each fertilized egg produces two polar bodies. The male and female pronuclei break down, and a series of cleavage divisions occur, rapidly forming an embryo. As more eggs are freed from the infusorigen, the older ones are pushed farther and farther from it. Thus a string of embryos may be produced in the axial cell of the rhombogen, in each direction from the infusorigen, with the fully developed infusoriforms at each end, then progressively younger embryos, and finally the infusorigen in the middle. This makes the study of the embryology of the infusoriform a delightful project to the biologist, since all the stages are seen lined up in proper sequence in a single parent rhombogen, and every cell can be studied without recourse to dissection or sectioning. This is much like the conditions seen in certain small nematodes in which the developing embryos can be clearly seen through the transparent body wall of their parent.

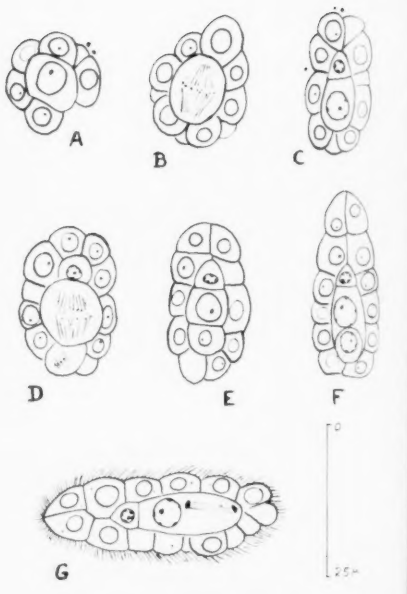


Fig. 8. Embryology of the vermiform larva of genus *Dicyemennea*. (*A*) Very young embryo showing the ... cell partially surrounded by somatic cells. (*B*) Slig... older embryo, showing first division of the axial cell. ... Resulting embryo, showing the abortive anterior axial ... and the larger functional axial cell. (*D*) Slightly ... embryo, showing the second division of the axial ... (*E*) Resulting embryo with a row of three internal ... the abortive anterior cell, the larger central axial cell, ... a posterior first germ cell. (*F*) Invasion of the axial ... by the first germ cell. (*G*) Older larva, showing ... of the first germ cell inside the axial cell.

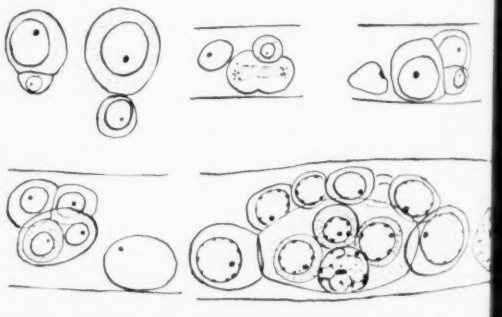


Fig. 9. Embryology of the infusorigen. (Top left) Eli... tion of the paranucleus—a small cell eliminated ... enlarged germ cell as the first step before it develops ... an infusorigen. This may correspond to the formatio... a small abortive cell in vermiform larvae of *Dicyemen*... In this case, the eliminated cell becomes a nucleus... body—the paranucleus—in the axial cell of the ... rhombogen. (Top center) Very young embryonic ... fusorigen and small paranucleus. (Top right) Sli... older embryo of an infusorigen. (Bottom left) Inva... of the axial cell of the infusorigen by the first ... (male) germ cell. (Bottom right) Young infusor... showing internal germ cells (male) in its axial cel... larger germ cells (female) at the periphery; those a... ends are enlarging and becoming egglike.

The Infusoriform

The infusoriform is a rounded animal about 1/40 mm long, corresponding in size to a small *Paramecium*. It has long strong cilia covering the posterior portion of its body, while the anterior part is occupied by two large refractile bodies of unknown function. Ventrally there is a pore leading into a hollowed-out internal cavity called the urn, in which are four large reproductive cells. Here again we meet the curious pattern of cells within cells. Each of the four cells within the urn contains two free nuclei of its own and one enclosed germ cell. Here then is a unique series of cells within cells:

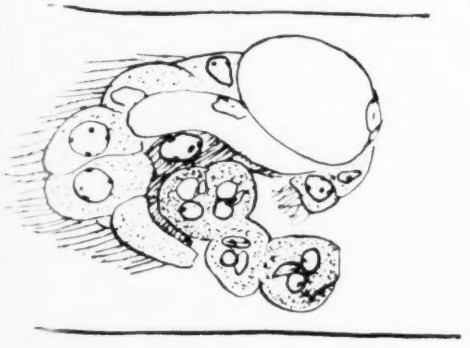
ide the axial cell of the parent rhombogen is its ture offspring, the infusorigen, giving rise to a rd type of individual, the infusoriform, which in n contains cells in which are enclosed the germ ls for a fourth generation (Fig. 10).

When fully developed, the infusoriform elabo- es a secretion from around its refractile bodies, hich causes the formation of a large swollen vacu- e in the axial cell of the rhombogen. This vacuole, ntaining the infusoriform, swells until it finally eaches the side of the rhombogen to the break- g point, whereupon the vacuole contents, includ- g the infusoriform itself, are squeezed out through side. Thus the rhombogen gives birth to its own andchild. The point of exit closes immediately, aving no visible scar on the rhombogen. There is special birth pore; the infusoriforms simply es- pe through the sides of the rhombogen wherever d whenever they are ready.

An infusorigen, after giving rise to many infu- riforms, eventually seems to be exhausted. It ases to produce sperm and eggs and degenerates til only the nucleus of its axial cell, now termed residual nucleus, remains in the parent rhom- gen.

The infusoriforms, once freed, leave the octopus, esumably being discharged in their host's urine. ey dive at once to the bottom of any container which they are placed and swim about rapidly. aquariums they live from 3 days to a week, by ich time most of them have discharged the germ ls from their urns and have begun to disintegrate.

Since the infusoriform is small and short lived, e it is very different in structure from the larval m nematogens that infect the very youngest phalopods, and since the infusoriform shows no terest in newly hatched octopuses or squids in quariums, it is assumed that it seeks out or is en- ifed by some bottom-living intermediate host, in



g. 10. Infusoriform discharging the four cells of the urn th their contained germ cells.

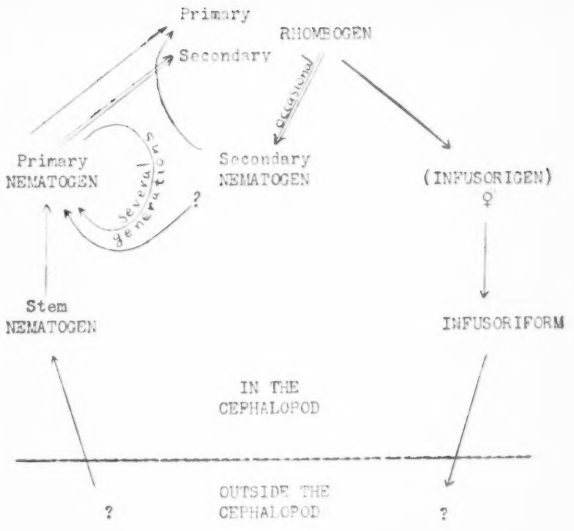


Fig. 11. Diagram of the known part of the life-cycle of the dicyemids as interpreted in this article.

which the intervening stages develop. This intermediate host must be very abundant, and the percentage of infection must be high, for along coasts where mesozoans occur it often appears that every octopus, almost without exception, is infected while it is still very young. All attempts to trace the fate of the infusoriform have failed. Elucidation of the portion of the life-cycle between liberation of the infusoriforms and infection of young octopuses remains a challenging problem.

The life-cycle of the dicyemids as here interpreted is summarized in Fig. 11. In this figure a single arrow indicates a new generation arising from germ cells in the preceding stage. A double arrow indicates that the change is merely a transformation of a given individual from one stage to another. Since rhombogens arise by both methods, a distinction is sometimes made. The ones that develop directly from larvae liberated during the time the population is undergoing the change of phase from nematogen to rhombogen and have, therefore, never previously functioned as nematogens are termed primary rhombogens. Those that result from the change of phase of adult nematogens are termed secondary rhombogens. In practice it is not easy to determine which category a given rhombogen belongs to. The distinction does not seem to be a very fundamental one but arises simply from the accident that the onset of the change of phase catches some individuals while they are young, others after they have already become mature nematogens.

The reader will also note in Fig. 11 an additional

stage not yet discussed—the secondary nematogen. Occasionally it seems that a few of the germ cells in mature nematogens persist unaltered through the period of rhombogen activity. When the rhombogen has ceased giving rise to infusoriforms, these cells may resume activity and produce a few vermiform larvae before the death of the old rhombogen.

In conclusion it will be seen that the dicyemid mesozoans exhibit a truly impressive number of unique peculiarities that fully justify their position as objects of special curiosity among zoologists, whether or not they are intermediate between the protozoans and higher animals, and in spite of the fact that they are of no economic importance.

They are multicellular animals with no trace of digestive, nervous, muscular, glandular, respiratory, or excretory systems.

They contain in their bodies single cells that reach 10 mm or more in length in some cases.

The embryos of all known stages of the life-cycle develop inside a single cell of their parents or of their grandparents, resulting in a condition of cells within cells to the fourth generation.

The sexual stages are reduced to a mere group of reproductive cells living their entire lives inside one of the cells of one of the asexual stages.

The rhombogens give birth not to their own offspring but to their grandchildren.

They live, usually in an otherwise pure and sterile culture, in the urine of octopuses and squids, hanging onto their host's kidney tissue, doing their host no apparent harm.

Part of the life-cycle still remains a challenging mystery.

Bibliography

This list includes only a few of the more important original works or reviews relating to the Mesozoa. More extended bibliographies can be found in some of them.

E. van Beneden, "Recherches sur les Dicyémides," *Bull. acad. roy. Belg., Ser. 2* **41,** 1160 (1876).

M. Gersch, "Der Entwicklungszyklus der Dicyemiden," *Z. wiss. Zoöl.* **151,** 515 (1938).

M. Hartmann, "Untersuchungen ueber den Generationswechsel der Dicyemiden," *Mém. acad. roy. Belg., Ser.* **17,** (1906).

L. H. Hyman, *The Invertebrates, Protozoa through Ctenophora.* (McGraw-Hill, New York, 1940).

A. Lameere, "Contribution a la connaissance des Dicyemides," *Bull. biol. France et Belg.* **50,** 1 (1916); 51, 3 (1918); **53,** 234 (1919).

B. H. McConnaughey, "The Life Cycle of the Dicyemid Mesozoa," *Univ. Calif. Publ. Zoöl.* **55,** 295 (1951).

H. Nouvel, "Les Dicyemides," *Arch. biol. (Paris)* 58, 3 (1947); **59,** 147 (1948).

C. O. Whitman, "A Contribution to the Embryology, Life History, and Classification of the Dicyemids," *Mitt. Zoöl. staatl. neapel* **4,** 1 (1883).

It is indeed wrong to think that the poetry of Nature's moods in all their infinite variety is lost on one who observes them scientifically, for the habit of observation refines our sense of beauty and adds a brighter hue to the richly coloured background against which each separate fact is outlined. The connection between events, the relation of cause and effect in different parts of a landscape, unite harmoniously what would otherwise be merely a series of detached scenes.—M. MINNAERT, *Light and Colour in the Open Air.*

The Sedentary Society

HERBERT COLLINS

The author is an assistant professor of social studies at North Carolina State College. This study is part of a larger project in which the evidence of science, technology, and art is used to document social and intellectual history. Articles that have already grown out of this project are "Scientific photography" and "The fabulous races."

THE seated figure of the 20th century is unmatched by any conventional posture in the past. In all their relationships, modern men spire to rationality, matter-of-factness, and objectivity. Their work requires reflection, observation, hand manipulation of buttons and switches. In these actions, chairs of one sort or another settle the worker into his sedentary occupation. With gantic mechanisms, operated from a sitting posion, the unwieldy phenomena of this world are anipulated. The derrick, bulldozer, and dredge erform tasks that far exceed ordinary human abilin any position other than sitting. There is even diminishing resistance to the union of buttocks nd machine in contemporary agriculture.

Whether sedentary workers supervise automatic achines, read dials and meters, or study molecur patterns, they are engaged in visual operations at require sitting. The extension and sensitization vision create situations in which instrument-using dividuals experience a new range of phenomena nd extend their control over previously unfamiliar bjects. Microscopists, radiologists, and meteorolosts are characteristic products of the mechanizaon of vision. In order to achieve these results, odern men have had to adjust to stationary posions, retinal concentration, and a seated posture. In airplanes or in tanks, the sedentary soldier exnds the military campaign to unprecedented disnces. The fixed individual of the sedentary society s in sports stadiums and becomes the renowned ectator who is actually our seated figure at play. o communicate he sits down to telephone or to pe a message. His thoughts then travel faster and rther than if he bodily moved himself. Instead walking to his destination, he motors or takes a ain. When he reserves passage on the next flight, distance to his destination exceeds the possibility body locomotion. In the pursuit of entertainment d recreation, multitudes of people view motion ctures from endless rows of seats. Or they remain home to operate dials on recorders, radios, televirs, or turntables.

Not only is the chair the conspicuous container modern men, but sitting is the symbolic posture of the age of science and technology. Since the earliest preliminaries of a quantitative, mechanical, and empirical approach to man, his environment, and his work, sedentary positions have been encouraged and sustained. Subsequently a stage in cultural development emerged in which man's characteristic act of bending himself and nature into a mutually harmonious relationship became assertive, and new outlets occurred for reflection and inquiry concerning objects of different shape, judgments of value and conduct, as well as poses and postures. Yet modern men have acquiesced in the view of an impersonal universe in which each individual has a seated station as stubborn as the facts out of which scientific theories have been fashioned.

A whole philosophy lies concealed in the act of sitting. The Renaissance merchant-bankers painted by Quentin Matsys, Hans Holbein the Younger, and Petrus Christus sat in abstract positions of quantitative preoccupation signalizing a new idea and posture (*1*). The symbols associated with the burghers were paper, writing instruments, books, coins, records, accounts, seals, scales, and secular possessions. These symbols of the external order of things show that the burghers thought the events and phenomena of this world were susceptible to measurement and human manipulation. The predecessors of the sedentary merchants were travelers, adventurers, sailors, itinerants, and craftsmen. When the techniques of business communication were perfected, with an array of ledgers and notations, mobility was outgrown and the sitting businessman was "born." Uppermost among his possessions were chairs, mirrors, and windows—the thrones, reflectors, and frames of a new social class. The orthodoxy of quantity that the merchants sponsored was appropriate to enumeration, analysis, standardization, and accumulation. Such specialized modes of behavior were also conducive to sitting.

The scientists who inquired into the nature of the universe contemporaneously with the merchants had also outgrown the mobility of earlier empiricism based on travel, exploration, and field observa-

tion. A new crop of scientists, whose observations became restricted to contrived experiments that a sedentary but constant observer could attend, supplanted the earlier mobile explorers. Even when assisted "by exquisite instruments," Francis Bacon relegated the senses "to judge of the experiment" which itself "shall judge of the thing" (*2*). Galileo, Huygens, Boyle, and Torricelli sat before their instruments of measurement and sight, created experimental situations, and altered man's view of the world without climbing Mount Ventoux. It was a field expedition to Cayenne with a simultaneous observation at Paris, in which Picard, Richer, and Cassini collaborated to secure a more accurate estimate of the earth's radius and the sun's mean distance from the earth, that enabled Isaac Newton to resume his researches on the inverse square law (*3*). Out of his mathematizing, without his traveling to the equator, emerged the theory of universal gravitation.

The archetype of experimental perseverance in the 17th century occurred in the iatrophysicist, Sanctus Sanctorius. In his *Ars de statica medicina*, he tells us that during the greater part of 30 years his life was "in the balance." He had spent a considerable part of his time in a weighing chair, with a table nearby, carefully recording variations in his weight under all sorts of conditions. He was able to show that the loss of weight by perspiration was greater than all "the servile Evacuations together" (*4*).

Other experimenters and philosophers, such as Harvey, Borelli, Descartes, and La Mettrie, contributed to the foundations of a mechanical view of the human organism. The method was derived from physics, the machine analogy was borrowed from mechanics, and the language echoed the matter-of-factness of mathematics. Descartes, for example, considered all the functions of the human body as proceeding "from the mere arrangement of its organs" as "do the movements of a clock, or other automaton, from that of its weights and wheels" (*5*). The geometrization of human nature was announced by Spinoza when he wrote that he would "regard human actions and desires exactly as if I were dealing with lines, planes, and bodies" (*6*).

Eventually physiological data, supported by clocks, tests, and efficiency ratings, became useful in factory management. By 1914 the quantification of work and effort had attained some precision. Jules Amar reported in *The Human Motor* (*7*):

> In the sitting position the base of support is much larger and the stability of the body is therefore considerably increased. Furthermore the muscles have but little to do. If we measure the relative consumption of oxygen by an adult, first standing in the easy attitude . . . and next sitting on a chair, leaning back with all the muscles of the legs relaxed, we shall find that the average economy of the consumption of oxygen is about 6% in favour of the sitting position.

Motivated by disinterested curiosity, employing a quantitative method of discovery, and in search of a new cosmic dramaturgy, the new scientists attained the same level of insight and expression as commercial men inspired by utility and expediency. The insights of both were governed by measurement and mathematical abstractions. According to Descartes (*8*):

> Mathematics excels in utility and simplicity the sciences subordinate to it, by the fact that it can deal with all the objects of which they have cognizance and many more besides.

This much at least the businessmen and scientists had in common at the outset of the modern age of quantity: the search for a mathematical image of reality permanently recorded on a piece of paper and arrived at from a sitting position. Lucas Pacioli, one of the early expounders of commercial arithmetic, aphorized 2 years after the discovery of America that care be taken "of the things that go by number of those which go by weight, and of those which go by measurement," and that "you give your daily attention to your affairs, chiefly holding the pen to paper and writing day by day that which you require" (*9*). Here was a discipline compatible to business as well as to research.

The discipline of quantitative callings was not, however, without basis in European culture. The Benedictine ideal that work should be tranfused with intellectual and moral vision linked knowledge and labor together. The Benedictine rule declared (*10*):

> Idleness is the enemy of the soul. And therefore, at fixed times, the brothers ought to be occupied in manual labor; and again, at fixed times, in sacred reading.

A portrayal of this ideal can be found in the medieval representation of the four Evangelists with their appropriate emblems prescriptively arranged around the Christ. Sometimes the Evangelists were separately chaired with book and writing instrument in hand, engrossed in divine labor. In order that nothing be omitted and all the symbols of communication, labor, and scholarship be seen, the masons included everything in the tympanum of the north portal of the Cathedral of Burgos: Christ enthroned and surrounded by angel, eagle, lion, and bull, and four desks and chairs with the Evangelists diligently bent over their work.

The Renaissance ideal did not, to be sure, center around the monastic laborer-scholar. Nevertheless, Arabic numerals, ledgers, weighing devices, clocks, and calipers arranged in a windowed room jointly symbolized the union of asceticism and worldliness. The appearance of portraiture, genre scenes, and lavishly seated Madonnas testified to the sedentary mannerism of a secular society. This is supported by the renditions of St. Jerome, the Christian scholar, translator, and bibliophile. When Lucas Cranach, Vittore Carpaccio, Domenico Ghirlandaio, and Albrecht Dürer, among others, portrayed the saint they did not take him literally: "Leave all, and come to the desert" (*11*). They reenacted the solitude of the wilderness and the cave in a four-walled setting, richly treated with books, paneling, furniture, and an assortment of instruments and scholarly gadgets. Here is the learned, contemplative man who must have a silent room and a chair to sit on. The ideals of the emerging sedentary society are being sanctified with the virtues of monasticism (*12*). These secular idealizations of the saint were matched by some of the depictions of the new science. In Dürer's "Melancholia" or Le Clerc's representation of the French Academy, geometric, mensural, and mechanical instruments and symbols were employed by seated or contemplative individuals on the verge of transforming knowledge and social life (*13*).

An advanced admirer of the sedentary society was Francis Bacon. Whatever the appraisal of his social scheme, he was an ameliorator of mankind. In his *New Atlantis*, Bacon delineated not only a technologic society but a way of life founded on scientific research. Somewhere in the hierarchy of technicians and experts he planned to place a group assigned to draw out of experiments "things of use and practice for man's life and knowledge as well as . . . for plain demonstration of causes" (*14*). Descartes also envisaged the time when, through science and its applications, men might be enabled to enjoy without any trouble the fruits of the earth" (*15*). For the first time, perhaps, the possibility occurred that science and technology might play into each other's hands. The myth maker met the artisan on common grounds, and scientific theories were used for practical ends. Under the auspices of scientists, processes of production were altered by phenomenal discoveries. A striking series of inventions, under the guidance of mechanicians, transformed production procedures. With the mingling of the two ways of knowing—on the one hand knowledge of what takes place and on the other hand knowledge of what had best be done learning and stubborn facts became indistinguishable (*16*).

This etching of "St. Jerome in His Study" is one of Albrecht Dürer's most famous masterpieces. [Reproduced by permission of Princeton University Press from *Albrecht Dürer*, by Erwin Panofsky]

What was novel about this arrangement was that it rapidly outmoded and even abandoned the traditional craftsman who had usually created something expressive of the potentialities of the union of hands, tools, and materials. The technology anticipated before the 19th century ignored the bodily motions of manualism. Instead, a quantitative approach to material resources in a setting of controlled research and dutiful matter-of-factness received approval. The manual operations of the workshop had been appropriated by the laboratory experimenter. As early as the 16th century, Andreas Vesalius, who was alert to the resources of the workshop, admonished anatomists to respect the practices of artisans (*17*). The abundance of mechanical draftsmanship and inventions, which could never have been realized without the direct aid of a craftsman or the cultivation of his mentality, thrived on workshop conditions and made the laboratory and the factory possible.

The artisan had been free and independent, terribly personal and haphazard. He had learned to depend on past experience without any quantitative method for the appraisal of the validity of the ways in which he acquired experience or

achieved workable results. But he was mobile. The discipline to which he was subjected was self-imposed or derived from his tools and materials. He would never have anticipated that higher predicament of freedom in which the machine process subordinates all human spontaneity and autonomy to the anonymous power of impersonal mechanism. Typing, calculating, machine assembly, sewing, visual inspection, and machine feeding—a partial list of services men perform from a sitting position —require skill and judgment. At the same time they are rationalized to conform to the performance of the machine.

The tests in use in personnel management, for all their value, signify subordination to the authority of impersonal numbers. Thomas A. Ryan claims in his book *Work and Effort* (*18*):

> Until we have measures which indicate the performances of the individual in the more complex activities involving thinking, imagining, comprehension, and emotion, we are bound to miss many of the elements of cost of work when we use the techniques of fatigue tests.

The point is, however, as Herbert Marcuse observed (*19*), that today

> . . . the apparatus to which the individual is to adjust and adapt is so rational that individual protest and liberation appear not only hopeless but as utterly irrational.

In this light Andrew Ure, the 19th century economist, no longer appears as a cruel exponent of the exploitation of the working class but as a pioneer observer of the realities of the mechanized society. Although Karl Marx had no patience with Ure and championed a different factory ideology, the 15th chapter in his *Capital* deserves attention (*20*):

> In handicrafts and manufactures, the workman makes use of a tool; in the factory the machine makes use of him. . . . In the factory we have a lifeless mechanism independent of the workman, who becomes its mere living appendage.

By the end of the 19th century there was something quaint about the merchant-adventurer, the explorer-naturalist, and the manual craftsman. During the century a few approximate examples emerged in a last spurt of the upright stance. The captain of industry, versed in many phases of commerce and industry, brought to bear on his situation a capacity for activity and a range of interests that kept him away from the executive office. Alexander von Humboldt, Charles Darwin, and the field archeologists and ethnologists retained a degree of mobility at odds with the laboratory anchorage of other scientists. William Morris and other craftsmen attempted to remind an obviously mechanized society of the primacy of esthetic standards and personal workmanship in the fashioning of useful goods. At the same time, however, the cash register, the typewriter, and eventually the telephone curtailed the wanderlust of the merchant. The research laboratory and the lecture stage where Humphry Davy, John Tyndall, and Thomas H. Huxley so eloquently held forth to a seated audience obscured the men who with rations and ideas ranged the unexplored remnants of the planet. Industrial production, located in castellated factories, fed by new knowledge of materials and processes, incorporating the designs of mechanical engineers, and staffed by routinized workers, eventually superseded the older traditions of individual workmanship. The monastic virtue of work had so permeated society that the administrators of punishment enthusiastically instituted penitentiary work.

The stage was set. From the thinnest of beginnings and within 300 years, a social revolution had fanned out over the Western world and its provinces. In numerous areas of activity mobility had succumbed to sitting. The man in demand is the sedentary, contemplative but worldly fellow, adept at figures, literate, and sitting apart from his neighbors. The table and desk have been mechanized. The chair acquires the standardized distinctions of the functions performed by the occupant. The merchant's desk has become an office of desks equipped with calculators, telephones, typewriters and punch-card machines requiring seated operators. A hierachy of seated workers replaces the mobile businessman ascending from the typist to the chairman of the board of directors. The scientist's desk has become an intricate laboratory with tubes, dials, and electronic equipment—a far cry from the alchemist's who also sat in the midst of a superabundance of paraphernalia. The artisan's table has become an automaton. The industrial plant has become the location for the rationalized flow of standarized production.

The concentration and attendance so consistently demanded by the machine have established the seated position as the postural model of the 20th century. "Everyone realizes the advantage, as a resting device, of anything upon which one can occasionally sit" (*21*). Frank Gilbreth, a pioneer advocating the seated posture in laboratory and factory, made that statement in 1916. Time, motion and fatigue analysis have gone far since his innovations. Yet the seated manipulation of the control panels of the machine process continues to receive serious attention. A management engineer meticulously writes (*22*):

With 40 inches taken as the average elbow height of the female worker, and with the hand allowed to work 1 to 3 inches lower than the elbow, the average height of the working surface should be 37 to 39 inches. The chair should be 25 to 31 inches, depending upon the proportions of the individual. With such table and chair heights the worker is permitted either to stand or sit at work, with the elbow and hand maintained at the same position relative to the work place.

Here we see how the quantitative method as applied to sedentary occupations has taken over even the act of sitting itself.

In this situation of transformation, a new diligence, a new attention to details, and a quantitative analysis of phenomena and men have triumphed. Never before have men known so much about the universe, have they produced with such abundance and precision, have they so rationally managed the components of social life. But never before have so many people sat so much of the time. Those who assume this posture, whatever their particular activities, are preoccupied with problems of concentration, assembly, repetition, restricted manual motions, visual attention, and relaxation. In these circumstances all men are equally situated. Their differences consist of degrees in intelligence, training, and posture.

The history of civilizations, although somewhat casually recapitulated in the construction and utilization of the chair, does not always record such equality in sitting. The chair is one of those objects man has made for human use by going beyond the human body. In doing this the materials employed have been molded into sympathy with the human body. All the efforts men have made to adapt themselves to the external world have required in one sense a departure from themselves. In another sense relatively formless materials have been sought which it has been the business of men to shape. Joseph Wood Krutch has indicated (*23*) how

> . . . man detaches himself from Nature, asserts the importance of himself as an individual, and proposes to himself ends and values which do not exist in her.

These projections of man, his body, and his personality into the physical universe are best realized and expressed by the implements and symbols that enhance his range of operations. The tools, clothing, utensils, shelters, furniture, and other modes of expression that characterize what it means to be human are all exemplary of a tendency to operate in what George Santayana called "an environment in which everything is not already what man is presently to make it" (*24*).

There have been and are civilizations to which the idea of the chair has never occured. The first appearance of detached seating furniture is seen in stools. Such circular, oval, oblong, curved, or hollowed-out seats served as buttocks props among pre-European South Americans, West Indians, and Africans. Not only did the earliest stools and joined chairs raise the body from the ground and provide outside support, but they served as adjuncts of social superiority, conveyed symbolic evidence of power, and were used as cult objects. In ancient Egypt, chair joinery became an organized craft. Mats sufficed in pre-Columbian Mexico for beds and seats. Many ancient civilizations produced thrones with legs and backs as honorific furniture. But not all sitting has been ceremonial. Leisure and comfort have sometimes been served by the furniture makers who have often produced sitting arrangements for the sheer object of turning out a decorative chair. Gothic choir stalls and lecterns, however, were straight, vertical, and narrow, built for ritual and never for comfort. The multitude either knelt or improvised seats.

In making the chair, the designer invents mechanical substitutes for the action of certain muscles to hold the body in a position other than supine. The solution of this problem dates from the use of furniture among sedentary people. No greater complexity faces the furniture designer than to construct a chair demanded by new habits of sitting and posture. The skeleton of the 18th century chair shows the designers' knowledge of lines of force, their insight into organic forms, and their mastery of wood. In the Bergere type, chair and posture became one. Siegfried Giedion has already displayed how this catering to the social conventions of sitting constituted one of the outstanding achievements of Rococo art (*25*). As the honorific associations of sitting waned, the chair speedily became the customary companion of whomsoever could afford to buy it. Almost at once the chair began to reflect the fashions of the hour, accommodating developments in clothes, hairdress, manners, and conversation. But the chair remained ostensibly a household article until industrial production asserted itself as a principle of social organization.

Although the 19th century became an upholsterer's holiday, producing mastodons with spring cushions and cylindrical arms, cushioned comfort was matched by maneuverability. Railroad, barber, typing, sewing, deck, invalid, surgical, and all manner of adjustable chairs were contrived to adjust the sitter to his tasks. Sitting was also rationalized in terms of engineering appraisals of the physiology of the seated posture. By constructing a shell as the container for a variously positionable body,

a resilient mode of sitting was achieved. The chair became the inverse image of the body, and the postural model of the body was ingeniously served by an endless variety of mechanical counterparts. However, the adjustability of the body that characterized some furniture of the last decades of the century did not always successfully compete with the bulk, clutter, ornamentation, and archaism sponsored by the ruling taste.

Not until our own time have bent wood, molded plywood, laminated woods, and foam rubber been adapted to flexible sitting. We are also witnessing the incorporation of cantilever seats in all manner of automatic, adjustable, and convertible chairs. Since the late 1850's the free-hovering steel cantilever seat has been associated with agricultural implements that have extended the mobility of the farmer. This perforated sheet of steel stamped as the container for the buttocks stands resiliently as the symbol of standardized and serialized comfort. Durable and repeatable, it has grace, contour, utility, and is the mechanical counterpart of organic seatability. The underlying principles in each of these designs have been a direct attack on the problem of sitting, a rational use of materials, and a fusion of function and human use.

No man, except the cataleptic, ever learned to sit without having to come to terms with either the ruling taste or the material techniques of his time. The Rietveld, Thonet, Hardoy, Eames, Breuer, and Mathsson chairs are a discipline requiring the same finesse demanded in the rocker, hammock, or swivel chair. In each case a new mode of sitting emerges in which the body and the chair collaborate. Actually chair design affords insight into historically prevailing accommodations for the contour of the human frame. In this sense, the chair is a multiple document of technology, design, and physiological science at any time. But the chair cannot contend unopposed for the singular distinction of determining the posture that every century evolves as its own. William Robertson, an 18th century Scottish historian, remarked during a discourse on the habits of the American Indian that posture "is an emblem of the state of his mind" (*26*). It might be added to his observation that posture is a visual record of the individual's body, his personality, the prevailing culture and the surrounding world. These are some of the primary experiences that constitute life and existence. Paul Schilder wrote (*27*):

> It is clear that every emotion expresses itself in the postural model of the body, and that every expressive attitude is connected with characteristic changes in the postural model of the body. . . . We expand and contract; we take parts away and we add parts; we rebuild it; we melt the details in; we create new details; we do this with our body and with the expression of the body itself.

And when the body is not sufficient for either playful or destructive changes in the "body image," tailoring, cosmetics, jewelry, and furniture have been invented to perform the task. Each age, furthermore, sees the body differently. The reclining nudes of Cranach, Giorgione, Titian, Ingres, Matisse, and Modigliani have symbolized characteristically different attitudes and conceptions of the human body. In medieval iconography, nudity was reserved for Adam and Eve as well as for lost souls condemned to Hell. After the 15th century others may disrobe; and if they choose, they may sit down too.

But to come back to the chair. It often symbolizes, as well as instigates, rules of decorum by encouraging squatting, kneeling, lounging, or sleeping. The chair may be in sympathy with or in opposition to the ruling taste and morality of a time. A British designer recently observed (*28*):

> Even though we no longer care to sit bolt upright, as our great-grandfathers did, and lounge a few inches nearer the floor than they would have thought consistent either with dignity or decency, we are no less concerned with posture (*28*).

During the last 100 years the chair has been severed from a restricted, honorific, ecclesiastical, academic and domestic anchorage. It has invaded such fields as recreation, entertainment, war, travel, work, communication, and research. Today those are the very situations in which value transformations are under way. These changes in the circumstances of living impress themselves on the postural model of the human body, which is itself in a continuous process of construction, reconstruction, and dissolution.

But only as men depart from the chair in all its variations do they achieve perfect comfort. The reclining figure, completely relaxed, antedates psychoanalysis and is perhaps the postural antidote to sitting. A recent commentator, after surveying pictorially the profiles of 30 chairs in various uses during the day, observed (*29*) that

> . . . as the sitter approaches perfect comfort, the designer's problem disappears, for, perfectly relaxed, the sitter finally transfers herself to a mattress on which she can lie perfectly straight.

This would indicate the failure of all mechanical contrivances to obscure the distinction between lying and sitting. But the sedentary society is a specialized arrangement of men and activities. It

was to be expected that this society would contrive mechanisms with finely varying distinctions for work and relaxation. But the emotional responses to the conditions of an industrial society have also produced a prolific variety of postural models, the most conspicuous of which has been sitting. Those who still stand must wait until the rationalizers of human posture provide them with the ubiquitous chair.

The age we inhabit has been shaped by science and the mechanical technology it breeds. It is no light responsibility that what men have been doing during the past 300 years will morally and intellectually influence many generations to come. For the sciences, however, no previous wisdom exists. They are no respecters of tradition, convention, or prescriptive canons of behavior. There is, nevertheless, nothing more devastating to thought and action than to drop with a minimum of discussion habitual concepts, attitudes, and beliefs. The revolution that science has been described as having ushered in actually was managed by those who refused to see themselves and their beliefs summarily dismissed. It must have been agonizing to dispute with opponents who possessed a standard of inquiry and truth that rested on heroic patience and refrained from

> . . . the din
> Of specious words, and tyranny of names. (*30*)

Yet the virtue of science is that it signalizes the first organizing and heuristic principle for the conduct of human affairs that willingly submits to the criticism of itself and the disturbances it provokes.

The early scientists fashioned a world-view at variance with traditional cosmologies, theories of man and nature, and modes of resources utilization. Moreover, they presided at the parturition of a new range of discourse, values, beliefs, manners, and social amenities. By becoming enmeshed in the social surroundings, the sciences have achieved the only kind of recognition that could have established their dominion. Together with other social activities, science constitutes one of the circumstances that make for history. And, like history, science remains inconclusive.

The cumulative and continous character of science is impressive, but the response of the social surroundings interjects strong strains of noncumulativeness and discontinuity that impose some of the inconclusiveness and ambiguity of history on science. It is often suggested that art is noncumulative and science is cumulative. These concepts are correct so far as the recurrence of styles and taste in art prevail and criterions of beauty remain subjective and indeterminate. The self-corrective aspects of science also confirm this distinction. But science displays discontinuous elements whenever it influences fashions and manners, turns from one set of assumptions to another, or insists on holding pet theories without empirical verification. The customary explanation of the time lag between Spallanzani and Pasteur in the refutation of the theory of spontaneous generation tends to drain the irony and contradiction from scientific endeavors.

The claim that an esthetic experience accompanies the practice of research and scientific theorizing is another instance of the temporal and intermittent range of scientific accomplishments. The experience is private, and the record of it is period literature. Indeed, the differentiation of science has resulted not only in a new range of knowledge but in creative work and new temporal circumstances. The most prolonged social circumstance of such origin has been the sedentary society.

On the other hand, there are certain exceptions to the claim that art is noncumulative. The rules of composition, the mediums employed, the technique, the vision of the artist and what he knows of color, light, and sight, as well as the knowledge all artists have of the factual content of human experience and the surrounding world, are cumulative and corrective rather than capricious.

We are at the point where the social contingencies rule the direction of science, which otherwise is quite poised and consistent in its investigations. All the tragedy, irony, and contradiction that men have experienced under other historical circumstances are still with us. On the cumulative side, science builds up a body of rational, verifiable knowledge; on its discontinuous side, it encourages a society in which heteronomous behavior becomes the condition of specialization, efficiency, and routine. In the sedentary society, sitting is substituted for the upright gait; the foot is restricted to the operation of a pedal; the hand is relieved of tools and employed in the operation of buttons and levers; the eye concentrates on dials and gages that substitute for the other organs of perception; and the brain, which is the seat of men's capacity to associate, remember, imagine, reflect, abstract, and think, asserts itself. But the line between intelligence and instinct, between reflection and repetition between choice and compliance, is so flimsy that the persuasive paradox of our time is that the sedentary society with all its conveniences resembles the termitary and the hive. As C. E. M. Joad has pointed out (*31*), the industrialized individual

> . . . is condemned by the conditions of his existence to perform with never varying efficiency operations of never varying monotony which conduce

to no end except the continued performance of similar operations in the future.

This can scarcely be the social analog of science. The scientists have dismissed most of the accumulated errors of men except their insights concerning themselves and their ways. Karl Mannheim once wrote (*32*):

> Just as nature was unintelligible to primitive man and his deepest feelings of anxiety arose from the incalculability of the forces of nature, so for modern industrialized man the incalculability of the forces at work in the social system under which he lives, with its social crises, inflation, and so on, has become a source of equally pervading fears.

The sedentary society is racked by these fears. It is devoted to the application of science in ways at odds with the principles of science. In the sedentary society, the concepts and findings of science are transformed into the mannerisms and patois of routinized and reticent men. To paraphrase E. E. Cummings, we are perpetually putting the secret of life in our pants and forgetting it is there and sitting down on it.

References and Notes

1. Q. Matsys, "The Banker and His Wife," in *Alinari Prints, a Thousand Paintings of Twenty Centuries* (International Art, Detroit, 1946), p. 135, pl. 1801; P. Christus, "St. Eloy," in *The Last Flowering of the Middle Ages*, J. Van der Elst (Doubleday, Garden City, 1945), pp. 68–69, pl. 26; H. Holbein, "The Merchant Giesze." in *Life's Picture History of Western Man* (Simon & Schuster, New York, 1951), p. 164. Also see L. C. Karpinski, *The History of Arithmetic*, (Rand McNally, Chicago, 1925), pp. 34, 58, 59; D. E. Smith *History of Mathematics*, (Ginn, Boston, 1925), vol. 1, pp. 334; vol. 2, pp. 180 182, 346.
2. E. A. Burtt, Ed., *The English Philosophers from Bacon to Mill* (Modern Library), p. 17.
3. A. Wolf, *A History of Science, Technology, and Philosophy in the 16th and 17th Centuries* (Allen & Unwin, London, 1950), pp. 175–176; H. Butterfield, *The Origins of Modern Science* (Bell, London, 1950), p. 140.
4. A. Wolf, *op. cit.*, pp. 433–434; J.F. Fulton, Ed., *Selected Readings in the History of Physiology* (Thomas, Springfield, Ill., 1930), pp. 140–145.
5. J. F. Fulton, Ed., *op. cit.* pp. 238–240; R. Descartes, *A Discourse on Method* (Everyman), pp. 40, 44, 46.
6. B. Spinoza, *Ethics* (Everyman), p. 84.
7. J. Amar, *The Human Motor* (Routledge, London, 1920), p. 338.
8. "Rules for the direction of the mind," in *Great Books of the Western World*, R. M. Hutchins, Ed. (Encyclopedia Britannica, Chicago, 1952), vol. 31, p. 7.
9. G. Bruun, *Europe in Evolution* (Houghton Mifflin, Boston, 1945), p. 103; Columbia University staff, *Introduction to Contemporary Civilization in the West, a Source Book* (Columbia University, Columbia Univ. Press, New York, 1947), vol. 1, p. 322; Karpinski, *op. cit.*, pp. 140–143; Smith, *op. cit.*, vol. 1, pp. 251–253.
10. *Introduction to Contemporary Civilization*, *op. cit.*, vol. 1, p. 139; A. N. Whitehead, *The Aims of Education* (Mentor), pp. 53–56.
11. H. O. Taylor, *The Medieval Mind* (Macmillan, London, 1938), vol. 1, p. 351, n. 1.
12. L. Cranach, "Cardinal Albrecht as St. Jerome," in *A Catalogue of Paintings in the John & Mabel Ringling Museum of Art*, W. E. Suida, Ed. (Sarasota, Fla., 1949), p. 257; V. Carpaccio, "St. Jerome in His Study," in *The Italian Painters of the Renaissance*, B. Berenson (Phaidon, New York, 1953), pl. 10; A. Dürer, "St. Jerome in His Study," in *Albrecht Dürer*, E. Panofsky (Princeton Univ. Press, Princeton, 1943), vol. 2, pl. 208; D. Ghirlandaio, "St. Jerome," in *An Illustrated Handbook of Art History*, F. J. Roos (Macmillan, New York, 1944), p. 128.
13. E. Panofsky, *op. cit.*, vol. 2, pl. 209; R. S. Stites, *The Arts and Man* (McGraw-Hill, New York, 1940), p. 641.
14. F. R. White, Ed., *Famous Utopias of the Renaissance* (Farrar, Straus, New York, 1948), p. 249.
15. R. Descartes, *op. cit.*, p. 49.
16. See T. Veblen, *The Place of Science in Modern Civilization and Other Essays* (Viking, New York, 1942), p. 18.
17. F. R. Moulton and J. J. Schifferes, Eds., *The Autobiography of Science* (Doubleday, Garden City, 1946), pp. 94–104.
18. T. A. Ryan, *Work and Effort, The Psychology of Production* (Ronald, New York, 1947), p. 153.
19. H. Marcuse, "Some social implications of modern technology," *Studies in Phil. and Social Sci.* 9 No. 3, 421, (1941).
20. K. Marx, *Capital, a Critique of Political Economy* (Modern Library) pp. 461–462.
21. F. B. Gilbreth, *Fatigue Study* (Macmillan, New York, 1919), p. 45.
22. R. M. Barnes, *Motion and Time Study* (Wiley, New York, 1949), p. 272. Also see the anthropometric study of railroad coach seating conducted by E. A. Hooton for the Haywood-Wakefield Co., *A Survey in Seating* (Gardner, Mass., 1954), *passim*.
23. J. W. Krutch, *The Modern Temper, a Study and a Confession* (Harcourt, Brace, New York, 1929), p. 36.
24. G. Santayana, *Life of Reason* (Scribner's, New York, 1923), vol. 4, p. 28.
25. S. Giedion, *Mechanization Takes Command* (Oxford Univ. Press, New York, 1948), pp. 309–319.
26. W. Robertson, *The History of the Discovery and Settlement of America* (Harper, New York, 1837), p. 188.
27. P. Schilder, *The Image and Appearance of the Human Body* (Kegan Paul, London, 1935), pp. 209, 210–211. Also see B. Rudofsky, *Are Clothes Modern? An Essay on Contemporary Apparel* (Paul Theobald, Chicago, 1947), *passim*.
28. J. Gloag, *Industrial Art Explained* (Allen & Unwin, London, 1946), pp. 183–184.
29. *Interiors and Industrial Design* **108**, 86 (1949).
30. J. Thomson (1700–48), "To the Memory of Sir Isaac Newton."
31. C. E. M. Joad, *Decadence, a Philosophical Inquiry* (Philosophical Library, New York, 1949), p. 39. The establishment of "uncontested knowledge" suggested to J. S. Mill the possibility of intellectual standardization for which he thought "negative philosophy" an antidote. Until people are pressed to examine the commonplaces of received opinion, "there will be few great thinkers . . . in any but the mathematical and physical branches of speculation." *On Liberty* (Everyman), pp. 103–105.
32. K. Mannheim, *Man and Society in an Age of Reconstruction* (Harcourt, Brace, New York, 1941), p. 59.

Psychoanalysis and Scientific Method

These four articles—by Else Frenkel-Brunswik, B. F. Skinner, Jerome Richfield, and Michael Scriven—are based by the authors on papers read by them in the symposium on "Psychoanalysis and scientific method" that comprised the third part of the five-part conference in the general subject Validation of Scientific Theories *held in Boston, Massachusetts, in December 1953. Prof. H. Feigl of the University of Minnesota was chairman for the symposium.*

The papers for the first two parts, on "Reasons for the acceptance of scientific theories" and "The present state of operationalism," appeared in the September and October issues. Those for the remaining parts, on "Organism and machine" and "Science as a social and historical phenomenon" will appear in subsequent issues.

Meaning of Psychoanalytic Concepts and Confirmation of Psychoanalytic Theories*

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FREUD'S ideas in the sphere of personality at first aroused a clamor of protest that has never entirely subsided. Once the initial shock resulting from Freud's discoveries had been overcome, however, the scrutinies of the system appear to be concerned more with its formal or methodological characteristics than with its content. Thus we hear of the alleged subjectivism or animism of psychoanalysis, of its confusion of hypotheses and facts, or of the nonverifiability of its hypotheses.

Many of the objections against psychoanalysis have their origin in an overly narrow interpretation of scientific empiricism or of operationism, and generally in a vaguely antitheoretical attitude. Since it is the physical sciences that are usually taken as the ideal model of scientific theory construction and of operational procedure, certain fundamental changes in the conception of theoretical structure that have taken place in the field of physics itself must be taken into consideration. Philipp Frank (*1*) points out that the earlier ultrapositivistic requirement, according to which all principles of physics should be formulated by using only observable qualities, has been broadened to include indirection; Einstein (*2*) speaks of "the ever-widening logical gap" between observation and basic concepts or laws. According to Hempel (*3*), it is precisely the "fictitious concepts rather than those fully definable by observables" that enable science to proceed to explanation and prediction.

A comparison between the situation in physics and in psychoanalysis is certainly not in all respects justified. However, modern physics and psychoanalysis have in common a turning away from the "natural" to a "fictitious" language. And the common result of this policy is that a wider and simpler network of interrelationships within observable data is ultimately being achieved. The fact that theoretical constructs, such as "unconsciousness," "id," "superego," or "repression," refer only indirectly, and not completely at that, to observable data must therefore not be made the basis of an objection against psychoanalysis as such. It may be helpful in the early stages of discovery to designate certain patterns of behavior in terms of the special and relatively fixed classifications listed. Today many of the earlier statements of psychoanalysis may be reformulated in terms of behavioral

patterns in such a manner that the facets of behavior connected with the more genuinely biological and instinctual processes—the id—are differentiated from those that are the result of cultural and parental commands and taboos—the superego.

Whereas in Europe the most important function of logical positivism was to stress the necessity of relating existing theories to empirical data, in this country, and especially in the case of the social sciences, its major function seems to be the advocacy of the formation of, and a tolerance for, theory per se.

With regard to the definition of basic concepts, some critics of psychoanalysis have objected to an alleged lack of sophistication in Freud concerning the philosophy of science and to his tendency to "reify" his concepts. Actually Freud, in contrast to some of his followers, was keenly aware of logical and epistemological problems. Definitions in science, he maintains (*4*, p. 60 f),

> . . . are in the nature of conventions; although everything depends on their being chosen in no arbitrary manner, but determined by the important relations they have to the empirical material—relations that we seem to divine before we can clearly recognize and demonstrate them. . . . Progressively we must modify these concepts so that they become widely applicable and at the same time consistent logically. Then, indeed, it may be time to immure them in definitions. . . . The science of physics furnishes an execellent illustration of the way in which even those "basic concepts" that are firmly established in the form of definitions are constantly being altered in their content.

Most clinical descriptions found in Freud employ the inferential construct of the "unconscious." Freud considers the assumption of unconsciousness as necessary because the data of consciousness are "exceedingly defective" (*5*, p. 22). Conscious acts alone do not enable us to account for certain aspects of slips of tongue and of other parapraxes, of dreams, of mental symptoms or obsessions in the sick, let alone the sudden inspirations of healthy persons. In carrying us, as Freud says, "beyond the limitations of direct experience," the assumption of unconscious acts makes the disconnected and unintelligible conscious acts fall into a demonstrable connection.

From the standpoint of the logic of science, unconscious tendencies are a special case of latent or "dispositional" characteristics. They are comparable to such physical characteristics as magnetism, provided that we do not insist on assigning them to "the mind" in a metaphysical sense. Such composite terms as "unconscious hostility" or "dependency" describe a disposition to display aggression or dependence under specified conditions, for example, in therapy. In his definition of behavior Carnap (*6*) has expressly included "dispositions to behavior which may not be manifest in a given special case." We therefore must agree with Freud that it is the very assumption of unconscious processes that enables psychoanalysis to take its place as "a natural science like any other" (*7*). He goes on to explain that these processes are "in themselves just as unknowable as those dealt with by other sciences such as physics and chemistry." And he remains in the spirit of the natural science when he stresses that "it is possible to establish the laws which those processes obey and to follow over long and unbroken stretches their mutual relation and interdependences" (p. 36).

Originally the concepts of conscious and unconscious signify particular "systems" possessed of certain dynamic characteristics, calling for a specification of their relationships within the over-all formal model. When dreams or subsequent free associations are used, this is done for the establishment of intermediate links that can be inserted in the gap between the two systems and that help to recover the latent material in a process of interpretation. Obviously to avoid confusion concerning mentalistic reification, Freud suggests "employing for the recognized mental systems certain arbitrarily chosen names." But since he cannot ignore consciousness as the common point of departure, he proposes to use the abbreviation Cs (for consciousness) and Ucs (for the uncounscious) when the two words are used in the systematic sense.

Only in Freud's later writings does the term "unconscious" take on a distinct reference to mental qualities. One of the chief reasons for this shift was the empirical realization that not only the id but also the superego is in part unconscious. In effect this merely underscores the increasing emphasis on the system-character of Freud's basic concepts and the decreasing emphasis on the more introspectionistic distinction between conscious and unconscious.

Freud readily acknowledges that a "rough correlation of . . . the mental apparatus to anatomy . . . exists." If so far every attempt to establish a localization of his constructs has miscarried, the present imperfect state of the biological sciences must be held responsible. Siegfried Bernfeld (*8*) has amply pointed out the influence of Helmholtz' physicalism and of the principle of conservation of energy on Freud. But it must be stressed that while at the beginning Freud was intensely dominated by neurophysiological thinking, the decisive progress in psychoanalysis did not occur until after he

freed himself from the search for such analogies and turned to more openly psychological models.

In defending the complexities of his approach, Freud stresses that there is no obligation to achieve at our very first attempt a theory that "commends itself by its simplicity, in which all is plain sailing." Freud argues that we must defend complexities of the theory itself so long as we find that they fit in with the results of observation; yet we must not abandon our expectation of being guided in the end by those very complexities to the recognition of "a state of affairs that is at once simple in itself and at the same time answers to all the complications of reality" (*9*, p. 122). If we note a similarity of tone with logical empiricism, we must not forget the fascination that in turn psychoanalysis has had for many of the logical empiricists; they have seen the genius of Freud at a time when most psychologists and psychiatrists were still deeply resistant.

Next to the concept of the unconscious, it is that of "instinct" which has been objected to most vigorously in the face of claims of psychoanalysis for consideration as a science. The psychoanalytic concept of instinct is complicated by the assumption of far-reaching transformations and disguises, particularly of the sex instinct. In reality, some parts of Freud's instinct theory, notably the theory of infantile sexuality and of the psychosexual stages of development, belong to the most lucid and most powerfully executed portions of the psychoanalytic system. As in the case of the unconscious, Freud pursues an essentially operational course in defining the instincts. He does so in pointing to the capacity of the instincts to "act vicariously for one another" and to readily change their objects. The mechanisms of repression, of reversal into the opposite, or of sublimation are some of the more striking examples of this variability. It may well be argued that the explanatory value of the concept of instinct lies precisely in this emphasis on variability. Only in the case of an assumed one-to-one correspondence between instinct and manifest behavior would the concept of instinct become circular or superfluous as an unnecessary duplication of behavior. Freud's concept of instinct is a truly explanatory, inferential construct imbued with some degree of independence. He avoided unnecessary duplication by fully considering the functional ambiguities inherent in the relationships between drives and behavior rather than by directly projecting behavioral trends back into the subjects.

The mixture of pioneering gusto with an understanding for ultimate logical requirements, which is so characteristic of Freud, is revealed when he speaks of the "superb indefiniteness" of the concept of instinct. He goes on to claim for the instincts "the same value as approximations as belongs to the corresponding intellectual scaffolding found in other natural sciences." We must expect them to "remain for a considerable time no less indeterminate than those of the older sciences (force, mass, attraction, etc.)" (*7*, p. 36).

Freud ascribes some of the difficulties in his speculations about the instincts—speculations that he likes to call his "mythology"—to our being obligated to operate with "metaphorical expressions peculiar to psychology." We must add in his behalf that, for the type of problems with which psychoanalysis deals, the mentalistic—introspectionist or animistic—vocabulary constitutes the precise counterpart to what Frank (*1*) calls the "pictural" vocabulary, and that in turn this latter vocabulary is recognized in physics as a legitimate or at least tolerable ingredient of the earlier stages of concept formation. Whereas the analogical procedure may not be suited for purposes of ultimately proving a scientific hypothesis, it may well be argued that the function of mentalistic analogies is more important in psychology than it is in physics.

One of the most bewildering aspects of psychoanalytic theory is the turning away from the obvious face-value picture of personality as it derives from introspection or from the direct, "phenotypical" observation of external behavior segments. An example is the reinterpretation of overt friendliness as a sign of underlying hostility, or of extreme tidiness as a sign of preoccupation with dirt. The discrepancy disappears with the specification of a set of fixed or variable operational conditions that determine when overt behavior is to be interpreted as "genuine" and when as manifesting some heterogeneous latent factor.

Since scientific inference concerning "central" processes—that is, the assumption of internal states on the basis of external evidence—cannot be defended unless it is based on a wide variety of circumstantial evidence (*10*), central inference can be said not to have been legitimately attempted before psychoanalysis. It can be shown that on the negative side of the ledger psychoanalysis, especially in its beginnings, has comparatively deemphasized both the surface manifestations in their specific identity and—what is more—the so-called "distal" achievements of behavior. These latter results of behavior in turn play the dominant role in Darwin's thinking and in such neo-behaviorist systems as that of Tolman (*11*). The regrouping of manifest observable facts as undertaken by Freud centers about sameness of "need"—that is, sameness of assumed internal cause or dynamism—while in the case of Tolman it centers about same-

ness of effect and, as we may add, in the case of Egon Brunswik's theory of perceptual thing-constancy (*12*) it centers about sameness of external object.

By virtue of this inherent incompleteness, psychoanalysis did not altogether manage to avoid the pitfalls of motivational relativism and of a genetic dissolution of overt adjustmental values. This one-sidedness has, to a certain extent, been remedied in the more recent turning of psychoanalysis from an almost exclusive emphasis on the id and on motivation to an increased concern with the ego—that is, with reality-oriented behavior and with adjustment in general. Even so, psychoanalytic expansion in this direction has been more programmatic than real, and there are a number of problems that can be solved only by an explicit integration of psychoanalysis with psychology proper and with sociology. The conceptual tools of psychoanalysis just are not sufficient to explain fully rational and social behavior (*10*). In fact, if we were to deny this we would obscure the essential theoretical contribution of Freud, which is his discovery of motivational dynamics.

In the context of adjustment problems, Freud tends to view character structure from a merely "defensive" point of view—that is, in terms of protecting oneself from internal threats rather than in terms of external task orientation—and social influences are seen as a series of traumata that bring to a halt or discontinue instinctual gratification and expression. While providing an understanding of an important aspect of the individual's attitude toward society, this view does not do justice to all the satisfactions gained from moving along constructive social avenues.

With all this said it must be granted that so far as motivation, per se, is concerned, psychoanalysis has achieved a legitimate reconstruction of objective causes rather than a mere pseudo-explanation in terms of subjectively experienced motives as such critics as Toulmin (*13*) and Flew (*14*) would have it. Far from identifying the introspectively reported motive and the objective explanation, the major merit of psychoanalysis is to have differentiated the two and unmasked and "discredited" as to their explanatory value the subjective experiences of motivation. The phenotypical, "manifest" characteristics are taken to provide only the indirect cues for inferences concerning the "latent," genotypical forces of motivation. It is of comparatively lesser significance that in the majority of cases it is verbal behavior such as dreams, free associations, and the like, rather than overt motor behavior proper, that psychoanalysis takes as the manifest basis for drive interpretations. This does not mean, however, that psychoanalysis is "introspectionistic." As everyone knows, it is precisely through psychoanalysis that we have learned to doubt the face value of introspection.

It must further be pointed out that the assumption of the dynamisms of the "inner man" to which such behaviorist critics of psychoanalysis as Skinner (*15*) have objected can be shown to increase the parsimony of the scientific description of behavior patterns. A translation of the psychoanalytic concepts into the terminology of the classical behaviorist's so-called "stimulus-response" approach, useful as it may be in certain contexts, has its difficulties and limitations. As we have seen, the major emphasis of psychoanalysis is on the discovery of internal causes; these include, in the language of psychoanalysis, "subjective phantasies" and generally the differential meanings an external event may acquire for various individuals. Freud began to make progress in his understanding of hysteria only after he had given up the idea of a simple external causation. Freud points out that only after the hypothetical factor of the hysterical phantasies had been introduced did the structure of the neurosis and its relationship to the patient "become conspicuous." Since the relationship of these phantasies to external factors is most complex and ambiguous, it seems heuristically fruitful to assume the internal mechanisms postulated by psychoanalysis, leaving their full operational specification for a latter time. Contrary to Skinner, I believe that such assumptions do not carry us outside the "bounds of natural science." But I do agree with Skinner on the point that any "looking inside the organism for an explanation of behavior" can easily lead to a neglect of some of the environmental factors and readily acknowledge that it has done so in the case of psychoanalysis.

Even more crucial is the fact that hypothetical extrapolations from overt behavior help to select the most relevant, though often less conspicuous, aspects of behavior which otherwise would be lost in the practically infinite range of possible observation. The relatively great explanatory and predictive value of hypotheses dealing with underlying motivation can be demonstrated statistically by means of multiple correlation (*16*). It is without doubt based on the fact that the selectivity just referred to enters crucially into the formation of these hypotheses. We may add that, from the standpoint of logical analysis, there is no alternative but to be behavioristic in any psychological endeavor; neither the so-called "subjective phan-

gists" in which psychoanalysis is interested, nor "introspective" events of any kind in others, can be constituted except by inference from the manifest physical observation of organisms.

In the process of theory-construction, Freud is generally quite careful in attempting to distinguish what we now call the "postulatory" from what we now call the "operational" elements of the theory, at the same time allowing their interplay as he moves along. However, his system would benefit from greater formalization and especially also from a more systematic differentiation between basic assumptions and their derivations. For example, a combination of the assumptions of infantile sexuality and of repression may be able to cover many of the more specific theorems in psychoanalysis.

Feigl (*17*) places psychoanalysis at the third of the four "levels" of explanation he distinguishes, thus grouping it together with the relatively descriptive behavior theories of Tolman and Hull. To me it seems that at least a certain group of psychoanalytic concepts, including that of the unconscious, goes beyond this level by involving what Reichenbach (*18*) calls "surplus meaning." In terms of a distinction recently injected into psychological theory by MacCorquodale and Meehl (*19*), this latter group of concepts would seem to be "hypothetical constructs," in contradistinction to the "intervening variables" which are thought of as resting exclusively on the values of a specified set of empirically observed data. In their own rather sketchy analysis, the last-named authors point out that such terms as "libido," "censorship," or "superego" were in psychoanalysis originally introduced as intervening variables—that is, as conventionalized designations of observable properties —but that there frequently was an unnoticed shift toward hypothetical constructs.

In their arguments the authors tend to overlook the fact that statements containing intervening variables are by no means exhaustible by statements concerning their observational basis. Both Carnap (*6*) and Hempel (*3*) have made it clear that sentences containing disposition terms cannot be fully translated into sentences about observables. Since we cannot specify all conditions and manners in which latent tendencies become manifest, dispositional statements involve "open" terms and require an infinite series of conditions in order to be tested.

The distinction between intervening variables and hypothetical constructs may, in my opinion, nonetheless be retained as a gradual one involving different degrees of indirectness of evidence or different kinds of surplus meaning. Possible relationships to the distinction made by Carnap and Hempel between postulatory theoretical constructs and concepts more directly reducible to observation could be pointed out. Guided by some relatively fragmentary initial empirical observations, Freud seems to have proceeded rather directly to the building of a hypothetical theoretical structure, with empirical interpretation lagging somewhat behind; in the definition of such theoretical constructs as superego, ego, and id, the major emphasis is on their structural relationships to one another rather than on their relationships to observation. His frequent oscillation between hypothetical constructs and intervening variables has afforded some protection against both a too narrow operationalism and the dangers of meaningless generalization.

Considering now briefly the attempts at confirmation of psychoanalytic hypotheses, it must be pointed out that by involving concepts more removed from the immediate data, psychoanalysis has lengthened the chains of intellectual and experimental work that connect the principles with the observational protocols. We may recall here the statement of Frank (*1*) that modern physics requires special ingenuity in verifying its theories and that this fact is a result of the greater abstractness of concepts. Traditional Newtonian physics could easily be verified by observation, since it was a direct formulation of everyday experience, obvious and plausible to common sense, whereas in Einstein's general theory of relativity "the description of the operations by which the quantities involved could be measured becomes a serious and complex task. It becomes an essential part of the theory" (*1*, p. 19 f).

Psychoanalysis shares with modern physics the fact that its statements do not lend themselves to the most direct and obvious types of confirmation. In each case, the highly interpretive statements involved do not carry the rules of their confirmation as obviously with themselves as do more descriptive statements. In reviewing the extensive literature on objective studies of psychoanalytic hypotheses (*20*, *21*), one is impressed by the fact that the more descriptive types of hypotheses involved in the theory of "fixation" and "regression" proved to be more readily accessible to experimental confirmation than the more explanatory ones on "repression," "projection," and "reaction formation." This may indeed be due to the fact that the latter derive from the more inferential and abstract parts of psychoanalytic theory. Complex conditions, such as those involved in the analysis of transference, are required before that which has been repressed may become conscious. Misunderstandings of psy-

choanalytic theory have arisen when statements concerning repression that originally were intended to refer to unconscious—that is, inferred rather than overt—processes, were erroneously taken as purely descriptive statements of conscious contents. As in physics, a simple identification of statements containing disposition terms with statements about manifest events is not permissible. Still and all, some of the experimental studies have verified even such seemingly far-fetched psychoanalytic assumptions as symbolism.

A type of approach other than the experimental, and one that I have tried to develop for a number of years, concentrates on the principle of alternative manifestations of motivational tendencies. This principle describes the basic pattern of interrelationships between the two strata involved in all psychoanalytic theory, the manifest and the latent, and can be shown to underlie most if not all of the specific mechanisms just mentioned. The possibility of analyzing statistically the tangled relationships between the two strata after imbuing them with some degree of operational independence may be illustrated by a study dealing with motivation in its relation to overt behavior segments (*16*) and one dealing with certain mechanisms of self-deception (*22*). In the former study a comparison of over-all motivational ratings with specific behavioral manifestations is used for a "rational reconstruction" of the cues underlying the so-called "intuitive inferences" made by the clinicians; the same general procedure would apply in case of the more explicit and more scrutinizing inferences concerning motivational dynamics made by the psychologist as a scientist rather than as a synoptic rater. Knowledge of the type of drive variable involved seems to hold good promise for behavior prediction of an "either-or" type, further specification of which must hinge on other than "dynamic" factors. Among these further factors determining, whether, say, underlying aggression is worked out in a socially constructive form or in neurotic symptoms, such situational factors as social and economic or occupational conditions must be assigned a major role.

In our study of self-deception, certain formal criteria of distortion, which may take their place alongside the more content-oriented type of diagnostic criteria favored in psychoanalysis proper, were established by means of a linguistic analysis of the individuals' responses. For example, favorable self-descriptions that do not correspond to the manifest behavioral realities are frequently formulated in exaggerated terms. The use of such linguistic or semantic devices as superlatives, generalizations, and repetitions was found to be statistically concomitant with a shortcoming rather than a strength in the area concerned.

In the verification of psychoanalytic hypotheses, the systematic evidence furnished by academic psychology constitutes only one of several avenues. Psychoanalysis itself has provided confirmatory, though seldom rigorous, empirical evidence of overwhelming scope, ranging from the wealth of material accumulated from individual patients to a synopsis of dream mechanisms, of lapses of tongue and memory, of pathologic symptoms, and of certain relevant features of folklore, myth, and other cultural phenomena. Regardless of how imperfect psychoanalytic theory may be in its formal structure, it has no rival among psychological theories as far as the range of both its evidence and its explanatory power is concerned.

Some of the obstacles encountered in the efforts to separate manifest behavior and latent motivation, or surface and depth in general, go beyond the merely methodological difficulties encountered in the process of scientific verification. One of these additional difficulties is a semantic one. The vocabulary of everyday language does not furnish us consistently with two separate sets of terms, one for overt behavior and the other for underlying motivation. Unless we drastically depart from familiar usage, the term "friendliness," for example, stands either for the basically friendly outlook on life or for the techniques of friendliness—genuine or fake—by which this basic outlook may be implemented or pretended, or for both. This dilemma is in a formal sense similar to the one presented by the two-faced meaning of our common perceptual terms (*12*). These terms also tend to have double reference, one to the personal and somewhat variable perceptual response, and the other to the interpersonal, measured physical stimulus. Most perceptual qualities exhibit highly tangled relationships to a variety of measured stimulus variables. The conceptual separation of perceptual stimulus and perceptual response can thus no longer appear as a case of entities superfluously multiplied; neither can, we may add, the separation of "behavior" and "motivation" with their similarly tangled relationships, as outlined in some of the foregoing paragraphs.

Certainly, both the motivations and the behaviors are constituted from overt behavior, as both stimuli and perceptual responses are constituted from different types of observational experiences. But motivations are arrived at through a synopsis of the constant elements in many bits of behavior. The problem of the genuineness of

behavior that I have pointed out as crucial in the context of proving psychoanalytic hypotheses illustrates the need that the two sets of events be made conceptually and operationally independent of each other so that their far-reaching actual independence under the principle of alternative manifestations can be brought out. An independent nomenclature for the different levels will thus in the end have to be established. This would remove much of the temptation to fall back into an oversimplified, prepsychoanalytic, single-level or surface treatment of the motivational aspects of behavior.

It has been observed that each time separations of the kind just described had to be substituted for previous identifications in the history of science, there was irrational, emotional resistance against the recognition of the equivocations or ambiguities involved. Besides their applicability to our outlook on psychoanalysis in particular, these resistances have some bearing on the topic of one of the other symposiums of this conference, the acceptance or rejection of scientific theories in general. They may also be linked with what I have called "intolerance of ambiguity" (*23*). Acceptance of the ambiguous relationship between motivation and manifestation, which is the chief discovery of psychoanalysis, requires cognitive tolerance of ambiguity on the part of the scientist. Its opposite, the concretistic, compulsive, and dogmatic patterns of perception and thought that have been so vividly described by both psychiatrists and psychoanalysts and are not infrequent among scientists are not conducive to the acceptance of psychoanalysis.

A final word must be added concerning the true or alleged ethical implications of psychoanalysis. Together with the cognitive resistances just outlined, ethical connotations may be the chief determinants of the acceptance and further destiny of any scientific theory. It has been objected against psychoanalysis—perhaps more often in the past than in the present—that its orientation is fundamentally amoral. Arguments of this kind were raised, not only by philosophers in search of a system of absolute values, but also by empirically oriented social scientists and psychologists of major stature, among them Max Weber (*24*). Weber saw in psychoanalysis an expression of a tendency to loosen our basic ethical principles. In a letter of 1907 Weber had accused Freud of proposing a psychiatric or "nerves" ethics characterized by the prevalence of the "hygienic" point of view (*25*, p. 417).

Against these strictures it may be said that, with all the reservations that psychoanalysis has voiced against an overly naive rational interpretation of ethics, it has merely turned against the assumed major executive principle of the traditional forms of ethics rather than against their basic constructive content. This particular executive principle is the mechanism of repression. Most prepsychoanalytic ethical systems stress such inhibitory devices as the looking away from evil, or its denial, or its mastery through strength of will. From psychoanalysis we have learned about the inefficiency and the dangers of these various forms of repression; from the same source we have learned of the importance of consciousness, integration, and maturity. All that is considered an essential ingredient of maturity in psychoanalysis, such as rationality, the overcoming of aggression, cooperativeness, the ability to love and to work, and the courage to face inside and outside threats that oppose these characteristics, bespeak standards that stand up well among the traditional systems of ethics. In psychoanalysis every neurosis is in and by itself considered as failure at moral control. The important historical contribution of traditional systems of ethics is the attempt to strengthen consciousness and conscience against the invasion of the instincts; through psychoanalysis we have become aware that such strengthening can be achieved only by facing and working through, rather than by merely condemning, the forces threatening our conscious personal and social values. From this latter viewpoint the mortal sin is self-deception and lack of insight rather than a lack of repression.

It may be that the diversion of attention from the functions of reason in psychoanalysis has contributed to the semblance of ethical relativism. As we have seen, psychoanalysis was so overwhelmed by its epoch-making discovery of the role of irrational forces that the explicit exploration of reasoning processes was temporarily obscured, even though it was reason and not the irrational that held the top spot so far as the evaluative attitude of psychoanalysis is concerned.

There is an illuminating reversal in the role played by reason when we compare the direct verbal formulations made by Freud, on the one hand, and Weber or Durkheim (*26*), on the other, with the actual function of reason in the theoretical edifices of these men. Both Durkheim and Weber have repeatedly been described as rationalists, albeit both see the foundations of society in fundamentally nonrational moral qualities. Freud, on the other hand, has been criticized for having given too much prominence to the irrational, while in fact his one hope is the overcoming of the irra-

tional in a society built on reason. Freud neglected to explore reason directly and challenged the potency of reason in guiding human conduct. But in his evaluations of the goals of human development he has an exalted esteem for reason, and his understanding for the vicissitudes of unreason has sharpened his grasp for the fundamental nature of reason; in this more crucial respect he is a believer in reason in the best sense of the word.

References and Notes

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1. P. Frank, *Modern Science and Its Philosophy* (Harvard Univ. Press, Cambridge, Mass., 1941).
2. A. Einstein, in *The Philosophy of Bertrand Russell*, Library of Living Philosophers, vol. 5, P. A. Schilpp, Ed. (Northwestern Univ. Press, Evanston, Ill., 1944), p. 289.
3. C. G. Hempel, *Fundamentals of concept formation in empirical science*, in *Intern. Encycl. Unified Sci.* vol. II, No. 7 (Univ. of Chicago Press, Chicago, 1952).
4. S. Freud, "Instincts and their vicissitudes" (1915), *Collected Papers* (Hogarth, London, 1925), vol. 4, pp. 60–83.
5. ———, "A note on the unconscious in psychoanalysis" (1912), *Collected Papers*, vol. 4, pp. 22–29.
6. R. Carnap, *Phil. Sci.* **3**, 420 (1936); **4**, 2 (1937).
7. S. Freud, *An Outline of Psychoanalysis* (1940), J. Strachey, Tr. (Norton, New York, 1949).
8. S. Bernfeld, *Psychoanal. Quart.* **13**, 341 (1944).
9. S. Freud, "The unconscious" (1915), *Collected Papers*, vol. 4, 98–136.
10. E. Frenkel-Brunswik, *J. Abnormal & Social Psychol.* **35**, 176 (1940).
11. E. C. Tolman, *Purposive Behavior in Animals and Men* (Century, New York, 1932).
12. E. Brunswik, *Wahrnehmung und Gegenstandswelt* (Deuticke, Vienna, 1934).
13. S. Toulmin, *Analysis* **9**, 23 (1948).
14. A. Flew, *ibid.* **10**, 8 (1949).
15. B. F. Skinner, *Science and Human Behavior* (Macmillan, New York, 1953).
16. E. Frenkel-Brunswik, *Motivation and Behavior* (Genetic Psychol. Monogr., 1942).
17. H. Feigl, in *Readings in Philosophical Analysis*, H. Feigl and W. Sellars, Eds. (Appleton-Century-Crofts, New York, 1949).
18. H. Reichenbach, *Experience and Prediction* (Univ. of Chicago Press, Chicago, 1938).
19. K. MacCorquodale and P. Meehl, *Psychol. Rev.* 55, 95 (1948).
20. E. R. Hilgard, in *Psychoanalysis as Science*, E. Pumpian-Mindlin, Ed. (Stanford Univ. Press, Calif., 1952).
21. R. R. Sears, *Survey of Objective Studies of Psychoanalytic Concepts* (Social Sci. Research Council Monogr. No. 51, 1943).
22. E. Frenkel-Brunswik, *J. Social Psychol.* **10**, 409 (1939).
23. ———, *J. Personality* **18**, 108 (1949).
24. Max Weber, *The Protestant Ethic and the Spirit of Capitalism* (Scribner's, New York, 1930).
25. Marianne Weber, *Max Weber: Ein Lebensbild* (Lambert-Schneider, Heidelberg, 1950).
26. E. Durkheim, *The Rules of Sociological Method* (Free Press, Glencoe, Ill., 1950).

Critique of Psychoanalytic Concepts and Theories

B. F. SKINNER

Dr. Skinner, who is professor of psychology at Harvard University, has contributed to the experimental analysis of behavior chiefly through work with rats and pigeons but recently he has extended his techniques to the human organism in the study of psychotic behavior and methods of instruction in elementary schools. Dr. Skinner received his training at Hamilton College and Harvard and has taught at the University of Minnesota and Indiana University. He is the author of Behavior of Organisms, Science and Human Behavior, *and* Walden Two *(a novel).*

FREUD'S great contribution to Western thought has been described as the application of the principle of cause and effect to human behavior. Freud demonstrated that many features of behavior hitherto unexplained—and often dismissed as hopelessly complex or obscure—could be shown to be the product of circumstances in the history of the individual. Many of the causal relationships he so convincingly demonstrated had been wholly unsuspected—unsuspected, in particular, by the very individuals whose behavior they controlled. Freud greatly reduced the sphere of accident and caprice in our considerations of human conduct. His achievement in this respect appears all the more impressive when we recall that he was never able to appeal to the quantitative proofs characteristic of other sciences. He carried the day with sheer persuasion—with the massing of instances

and the delineation of surprising parallels and analogies among seemingly diverse materials.

This was not, however, Freud's own view of the matter. At the age of 70 he summed up his achievement in this way (*1*):

> My life has been aimed at one goal only: to infer or guess how the mental apparatus is constructed and what forces interplay and counteract in it.

It is difficult to describe the mental apparatus he refers to in noncontroversial terms, partly because Freud's conception changed from time to time and partly because its very nature encouraged misinterpretation and misunderstanding. But it is perhaps not too wide of the mark to indicate its principal features as follows: Freud conceived of some realm of the mind, not necessarily having physical extent, but nevertheless capable of topographic description and of subdivision into regions of the conscious, coconscious, and unconscious. Within this space, various mental events—ideas, wishes, memories, emotions, instinctive tendencies, and so on—interacted and combined in many complex ways. Systems of these mental events came to be conceived of almost as subsidiary personalities and were given proper names: the id, the ego, and the superego. These systems divided among themselves a limited store of psychic energy. There were, of course, many other details.

No matter what logicians may eventually make of this mental apparatus, there is little doubt that Freud accepted it as real rather than as a scientific construct or theory. One does not at the age of 70 define the goal of one's life as the exploration of an explanatory fiction. Freud did not use his "mental apparatus" as a postulate system from which he deduced theorems to be submitted to empirical check. If there was any interaction between the mental apparatus and empirical observations, it took the form of modifying the apparatus to account for newly discovered facts. To many followers of Freud the mental apparatus appears to be equally as real, and the exploration of such an apparatus is similarly accepted as the goal of a science of behavior. There is an alternative view, however, which holds that Freud did not discover the mental apparatus but rather invented it, borrowing part of its structure from a traditional philosophy of human conduct but adding many novel features of his own devising.

There are those who will concede that Freud's mental apparatus was a scientific construct rather than an observable empirical system but who, nevertheless, attempt to justify it in the light of scientific method. One may take the line that metaphorical devices are inevitable in the early stages of any science and that although we may look with amusement today upon the "essences," "forces," "phlogistons," and "ethers," of the science of yesterday, these nevertheless were essential to the historical process. It would be difficult to prove or disprove this. However, if we have learned anything about the nature of scientific thinking, if mathematical and logical researches have improved our capacity to represent and analyze empirical data, it is possible that we can avoid some of the mistakes of adolescence. Whether Freud could have done so is past demonstrating, but whether we need similar constructs in the future prosecution of a science of behavior is a question worth considering.

Constructs are convenient and perhaps even necessary in dealing with certain complicated subject matters. As Frenkel-Brunswik shows (*2*), Freud was aware of the problems of scientific methodology and even of the metaphorical nature of some of his own constructs. When this was the case, he justified the constructs as necessary or at least highly convenient. But awareness of the nature of the metaphor is no defense of it, and if modern science is still occasionally metaphorical, we must remember that theory-wise it is also still in trouble. The point is not that metaphor or construct is objectionable but that particular metaphors and constructs have caused trouble and are continuing to do so. Freud recognized the damage worked by his own metaphorical thinking, but he felt that it could not be avoided and that the damage must be put up with. There is reason to disagree with him on this point.

Freud's explanatory scheme followed a traditional pattern of looking for a cause of human behavior inside the organism. His medical training supplied him with powerful supporting analogies. The parallel between the excision of a tumor, for example, and the release of a repressed wish from the unconscious is quite compelling and must have affected Freud's thinking. Now, the pattern of an inner explanation of behavior is best exemplified by doctrines of animism, which are primarily concerned with explaining the spontaneity and evident capriciousness of behavior. The living organism is an extremely complicated system behaving in an extremely complicated way. Much of its behavior appears at first blush to be absolutely unpredictable. The traditional procedure had been to invent an inner determiner, a "demon," "spirit," "homunculus," or "personality" capable of spontaneous change of course or of origination of action. Such an inner determiner offers only a momentary explanation of the behavior of the outer organism,

tional in a society built on reason. Freud neglected to explore reason directly and challenged the potency of reason in guiding human conduct. But in his evaluations of the goals of human development he has an exalted esteem for reason, and his understanding for the vicissitudes of unreason has sharpened his grasp for the fundamental nature of reason; in this more crucial respect he is a believer in reason in the best sense of the word.

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3. C. G. Hempel, *Fundamentals of concept formation in empirical science,* in *Intern. Encycl. Unified Sci.* vol. II, No. 7 (Univ. of Chicago Press, Chicago, 1952).
4. S. Freud, "Instincts and their vicissitudes" (1915), *Collected Papers* (Hogarth, London, 1925), vol. 4, pp. 60–83.
5. ———, "A note on the unconscious in psychoanalysis" (1912), *Collected Papers,* vol. 4, pp. 22–29.
6. R. Carnap, *Phil. Sci.* **3**, 420 (1936); **4**, 2 (1937).
7. S. Freud, *An Outline of Psychoanalysis* (1940), J. Strachey, Tr. (Norton, New York, 1949).
8. S. Bernfeld, *Psychoanal. Quart.* **13**, 341 (1944).
9. S. Freud, "The unconscious" (1915), *Collected Papers,* vol. 4, 98–136.
10. E. Frenkel-Brunswik, *J. Abnormal & Social Psychol.* **35**, 176 (1940).
11. E. C. Tolman, *Purposive Behavior in Animals and Men* (Century, New York, 1932).
12. E. Brunswik, *Wahrnehmung und Gegenstandswelt* (Deuticke, Vienna, 1934).
13. S. Toulmin, *Analysis* **9**, 23 (1948).
14. A. Flew, *ibid.* **10**, 8 (1949).
15. B. F. Skinner, *Science and Human Behavior* (Macmillan, New York, 1953).
16. E. Frenkel-Brunswik, *Motivation and Behavior* (Genetic Psychol. Monogr., 1942).
17. H. Feigl, in *Readings in Philosophical Analysis,* H. Feigl and W. Sellars, Eds. (Appleton-Century-Crofts, New York, 1949).
18. H. Reichenbach, *Experience and Prediction* (Univ. of Chicago Press, Chicago, 1938).
19. K. MacCorquodale and P. Meehl, *Psychol. Rev.* **55**, 95 (1948).
20. E. R. Hilgard, in *Psychoanalysis as Science,* E. Pumpian-Mindlin, Ed. (Stanford Univ. Press, Calif., 1952).
21. R. R. Sears, *Survey of Objective Studies of Psychoanalytic Concepts* (Social Sci. Research Council Monogr. No. 51, 1943).
22. E. Frenkel-Brunswik, *J. Social Psychol.* **10**, 409 (1939).
23. ———, *J. Personality* **18**, 108 (1949).
24. Max Weber, *The Protestant Ethic and the Spirit of Capitalism* (Scribner's, New York, 1930).
25. Marianne Weber, *Max Weber: Ein Lebensbild* (Lambert-Schneider, Heidelberg, 1950).
26. E. Durkheim, *The Rules of Sociological Method* (Free Press, Glencoe, Ill., 1950).

Critique of Psychoanalytic Concepts and Theories

B. F. SKINNER

Dr. Skinner, who is professor of psychology at Harvard University, has contributed to the experimental analysis of behavior chiefly through work with rats and pigeons but recently he has extended his techniques to the human organism in the study of psychotic behavior and methods of instruction in elementary schools. Dr. Skinner received his training at Hamilton College and Harvard and has taught at the University of Minnesota and Indiana University. He is the author of Behavior of Organisms, Science and Human Behavior, *and* Walden Two *(a novel).*

FREUD'S great contribution to Western thought has been described as the application of the principle of cause and effect to human behavior. Freud demonstrated that many features of behavior hitherto unexplained—and often dismissed as hopelessly complex or obscure—could be shown to be the product of circumstances in the history of the individual. Many of the causal relationships he so convincingly demonstrated had been wholly unsuspected—unsuspected, in particular, by the very individuals whose behavior they controlled. Freud greatly reduced the sphere of accident and caprice in our considerations of human conduct. His achievement in this respect appears all the more impressive when we recall that he was never able to appeal to the quantitative proofs characteristic of other sciences. He carried the day with sheer persuasion—with the massing of instances

and the delineation of surprising parallels and analogies among seemingly diverse materials.

This was not, however, Freud's own view of the matter. At the age of 70 he summed up his achievement in this way (*1*):

> My life has been aimed at one goal only: to infer or guess how the mental apparatus is constructed and what forces interplay and counteract in it.

It is difficult to describe the mental apparatus he refers to in noncontroversial terms, partly because Freud's conception changed from time to time and partly because its very nature encouraged misinterpretation and misunderstanding. But it is perhaps not too wide of the mark to indicate its principal features as follows: Freud conceived of some realm of the mind, not necessarily having physical extent, but nevertheless capable of topographic description and of subdivision into regions of the conscious, coconscious, and unconscious. Within this space, various mental events—ideas, wishes, memories, emotions, instinctive tendencies, and so on—interacted and combined in many complex ways. Systems of these mental events came to be conceived of almost as subsidiary personalities and were given proper names: the id, the ego, and the superego. These systems divided among themselves a limited store of psychic energy. There were, of course, many other details.

No matter what logicians may eventually make of this mental apparatus, there is little doubt that Freud accepted it as real rather than as a scientific construct or theory. One does not at the age of 70 define the goal of one's life as the exploration of an explanatory fiction. Freud did not use his "mental apparatus" as a postulate system from which he deduced theorems to be submitted to empirical check. If there was any interaction between the mental apparatus and empirical observations, it took the form of modifying the apparatus to account for newly discovered facts. To many followers of Freud the mental apparatus appears to be equally as real, and the exploration of such an apparatus is similarly accepted as the goal of a science of behavior. There is an alternative view, however, which holds that Freud did not discover the mental apparatus but rather invented it, borrowing part of its structure from a traditional philosophy of human conduct but adding many novel features of his own devising.

There are those who will concede that Freud's mental apparatus was a scientific construct rather than an observable empirical system but who, nevertheless, attempt to justify it in the light of scientific method. One may take the line that metaphorical devices are inevitable in the early stages of any science and that although we may look with amusement today upon the "essences," "forces," "phlogistons," and "ethers," of the science of yesterday, these nevertheless were essential to the historical process. It would be difficult to prove or disprove this. However, if we have learned anything about the nature of scientific thinking, if mathematical and logical researches have improved our capacity to represent and analyze empirical data, it is possible that we can avoid some of the mistakes of adolescence. Whether Freud could have done so is past demonstrating, but whether we need similar constructs in the future prosecution of a science of behavior is a question worth considering.

Constructs are convenient and perhaps even necessary in dealing with certain complicated subject matters. As Frenkel-Brunswik shows (*2*), Freud was aware of the problems of scientific methodology and even of the metaphorical nature of some of his own constructs. When this was the case, he justified the constructs as necessary or at least highly convenient. But awareness of the nature of the metaphor is no defense of it, and if modern science is still occasionally metaphorical, we must remember that theory-wise it is also still in trouble. The point is not that metaphor or construct is objectionable but that particular metaphors and constructs have caused trouble and are continuing to do so. Freud recognized the damage worked by his own metaphorical thinking, but he felt that it could not be avoided and that the damage must be put up with. There is reason to disagree with him on this point.

Freud's explanatory scheme followed a traditional pattern of looking for a cause of human behavior inside the organism. His medical training supplied him with powerful supporting analogies. The parallel between the excision of a tumor, for example, and the release of a repressed wish from the unconscious is quite compelling and must have affected Freud's thinking. Now, the pattern of an inner explanation of behavior is best exemplified by doctrines of animism, which are primarily concerned with explaining the spontaneity and evident capriciousness of behavior. The living organism is an extremely complicated system behaving in an extremely complicated way. Much of its behavior appears at first blush to be absolutely unpredictable. The traditional procedure had been to invent an inner determiner, a "demon," "spirit," "homunculus," or "personality" capable of spontaneous change of course or of origination of action. Such an inner determiner offers only a momentary explanation of the behavior of the outer organism,

because it must, of course, be accounted for also, but it is commonly used to put the matter beyond further inquiry and to bring the study of a causal series of events to a dead end.

Freud, himself, however, did not appeal to the inner apparatus to account for spontaneity or caprice because he was a thoroughgoing determinist. He accepted the responsibility of explaining, in turn, the behavior of the inner determiner. He did this by pointing to hitherto unnoticed external causes in the environmental and genetic history of the individual. He did not, therefore, need the traditional explanatory system for traditional purposes; but he was unable to eliminate the pattern from his thinking. It led him to represent each of the causal relationships he had discovered as a series of three events. Some environmental condition, very often in the early life of the individual, leaves an effect upon the inner mental apparatus, and this in turn produces the behavioral manifestation or symptom. Environmental event, mental state or process, behavioral symptom—these are the three links in Freud's causal chain. He made no appeal to the middle link to explain spontaneity or caprice. Instead he used it to bridge the gap in space and time between the events he had proved to be causally related.

A possible alternative, which would have had no quarrel with established science, would have been to argue that the environmental variables leave *physiological* effects that may be inferred from the behavior of the individual, perhaps at a much later date. In one sense, too little is known at the moment of these physiological processes to make them useful in a legitimate way for this purpose. On the other hand, too much is known of them, at least in a negative way. Enough is known of the nervous system to place certain dimensional limits upon speculation and to clip the wings of explanatory fiction. Freud accepted, therefore, the traditional fiction of a mental life, avoiding an out-and-out dualism by arguing that eventually physiological counterparts would be discovered. Quite apart from the question of the existence of mental events, let us observe the damage that resulted from this maneuver.

We may touch only briefly upon two classical problems that arise once the conception of a mental life has been adopted. The first of these is to explain how such a life is to be observed. The introspective psychologists had already tried to solve this problem by arguing that introspection is only a special case of the observation upon which all science rests and that man's experience necessarily stands between him and the physical world with which science purports to deal. But it was Freud himself who pointed out that not all of one's mental life was accessible to direct observation—that many events in the mental apparatus were necessarily inferred. Great as this discovery was, it would have been still greater if Freud had taken the next step, advocated a little later by the American movement called Behaviorism, and insisted that conscious, as well as unconscious, events were inferences from the facts. By arguing that the individual organism simply reacts to its environment, rather than to some inner experience of that environment, the bifurcation of nature into physical and psychic can be avoided (*3*).

A second classical problem is how the mental life can be manipulated. In the process of therapy, the analyst necessarily acts upon the patient only through physical means. He manipulates variables occupying a position in the first link of Freud's causal chain. Nevertheless, it is commonly assumed that the mental apparatus is being directly manipulated. Sometimes it is argued that processes are initiated within the individual himself, such as those of free association and transference, and that these in turn act directly upon the mental apparatus. But how are these mental processes initiated by physical means? The clarification of such a causal connection places a heavy and often unwelcome burden of proof upon the shoulders of the dualist.

The important disadvantages of Freud's conception of mental life can be described somewhat more specifically. The first of these concerns the environmental variables to which Freud so convincingly pointed. The cogency of these variables was frequently missed because the variables were transformed and obscured in the course of being represented in mental life. The physical world of the organism was converted into conscious and unconscious experience, and these experiences were further transmuted as they combined and changed in mental processes. For example, early punishment of sexual behavior is an observable fact that undoubtedly leaves behind a changed organism. But when this change is represented as a state of conscious or unconscious anxiety or guilt, specific details of the punishment are lost. When, in turn, some unusual characteristic of the sexual behavior of the adult individual is related to the supposed guilt, many specific features of the relationship may be missed that would have been obvious if the same features of behavior had been related to the punishing episode. Insofar as the mental life of the individual is used as Freud used it to represent and

to carry an environmental history, it is inadequate and misleading.

Freud's theory of the mental apparatus had an equally damaging effect upon his study of behavior as a dependent variable. Inevitably, it stole the show. Little attention was left to behavior per se. Behavior was relegated to the position of a mere mode of expression of the activities of the mental apparatus or the symptoms of an underlying disturbance. Among the problems not specifically treated in the manner that was their due, we may note five.

1) The nature of the act as a unit of behavior was never clarified. The simple *occurrence* of behavior was never well represented. "Thoughts" could "occur" to an individual; he could "have" ideas according to the traditional model; but he could "have" behavior only in giving expression to these inner events. We are much more likely to say that "the thought occurred to me to ask him his name" than that "the act of asking him his name occurred to me." It is in the nature of thoughts and ideas that they occur to people, but we have never come to be at home in describing the emission of behavior in a comparable way. This is especially true of verbal behavior. In spite of Freud's valuable analysis of verbal slips and of the techniques of wit and verbal art, he rejected the possibility of an analysis of verbal behavior in its own right rather than as the expression of ideas, feelings, or other inner events, and therefore missed the importance of this field for the analysis of units of behavior and the conditions of their occurence.

The behavioral nature of perception was also slighted. To see an object as an object is not mere passive sensing; it is an act, and something very much like it occurs when we see an object although no object is present. Fantasy and dreams were for Freud not the perceptual *behavior* of the individual but pictures painted by an inner artist in some atelier of the mind which the individual then contemplated and perhaps then reported. This division of labor is not essential when the behavioral component of the act of seeing is emphasized.

2) The dimensions of behavior, particularly its dynamic properties, were never adequately represented. We are all familiar with the fact that some of our acts are more likely to occur upon a given occasion than others. But this likelihood is hard to represent and harder to evaluate. The dynamic changes in behavior that are the first concern of the psychoanalyst are primarily changes in probability of action. But Freud chose to deal with this aspect of behavior in other terms—as a question of "libido," "cathexis," "volume of excitation," "instinctive or emotional tendencies," "available quantities of psychic energy," and so on. The delicate question of how probability of action is to be quantified was never answered, because these constructs suggested dimensions to which the quantitative practices of science in general could not be applied.

3) In his emphasis upon the genesis of behavior, Freud made extensive use of processes of learning. These were never treated operationally in terms of changes in behavior but rather as the acquisition of ideas, feelings, and emotions later to be expressed by, or manifested in, behavior. Consider, for example, Freud's own suggestion that sibling rivalry in his own early history played an important part in his theoretical considerations as well as in his personal relationships as an adult.

An infant brother died when Freud himself was only 1½ years old, and as a young child Freud played with a boy somewhat older than himself and presumably more powerful, yet who was, strangely enough, in the nominally subordinate position of being his nephew. To classify such a set of circumstances as sibling rivalry obscures, as we have seen, the many specific properties of the circumstances themselves regarded as independent variables in a science of behavior. To argue that *what was learned* was the effect of these circumstances upon unconscious or conscious aggressive tendencies or feelings of guilt works a similar misrepresentation of the dependent variable. An emphasis upon behavior would lead us to inquire into the specific acts plausibly assumed to be engendered by these childhood episodes. In very specific terms, how was the behavior of the young Freud *shaped* by the special reinforcing contingencies arising from the presence of a younger child in the family, by the death of that child, and by later association with an older playmate who nevertheless occupied a subordinate family position? What did the young Freud *learn to do* to achieve parental attention under these difficult circumstances? How did he avoid aversive consequences? Did he exaggerate any illness? Did he feign illness? Did he make a conspicuous display of behavior that brought commendation? Was such behavior to be found in the field of physical prowess or intellectual endeavor? Did he learn to engage in behavior that would in turn increase the repertoires available to him to achieve commendation? Did he strike or otherwise injure young children? Did he learn to injure them verbally by teasing? Was he punished for this, and if so, did he discover other forms of behavior that had the same damaging effect but were immune to punishment?

We cannot, of course, adequately answer ques-

tions of this sort at so late a date, but they suggest the kind of inquiry that would be prompted by a concern for the *explicit shaping of behavioral repertoires* under childhood circumstances. What has survived through the years is not aggression and guilt, later to be manifested in behavior, but rather patterns of behavior themselves. It is not enough to say that this is "all that is meant" by sibling rivalry or by its effects upon the mental apparatus. Such an expression obscures, rather than illuminates, the nature of the behavioral changes taking place in the childhood learning process. A similar analysis could be made of processes in the fields of motivation and emotion.

4) An explicit treatment of behavior as a datum, of probability of response as the principal quantifiable property of behavior, and of learning and other processes in terms of changes of probability is usually enough to avoid another pitfall into which Freud, in common with his contemporaries, fell. There are many words in the layman's vocabulary that suggest the activity of an organism yet are not descriptive of behavior in the narrower sense. Freud used many of these freely—for example, the individual is said to discriminate, remember, infer, repress, decide, and so on. Such terms do not refer to specific acts. We say that a man discriminates between two objects when he behaves differently with respect to them; but discriminating is not itself behavior. We say that he represses behavior which has been punished when he engages in other behavior *just because* it displaces the punished behavior; but repressing is not action. We say that he decides upon a course of conduct either when he enters upon one course to the exclusion of another, or when he alters some of the variables affecting his own behavior in order to bring this about; but there is no other "act of deciding." The difficulty is that when one uses terms which suggest an activity, one feels it necessary to invent an actor, and the subordinate personalities in the Freudian mental apparatus do, indeed, participate in just these activities rather than in the more specific behavior of the observable organism.

Among these activities are conspicuous instances involving the process of self-control—the so-called "Freudian mechanisms." These need not be regarded as activities of the individual or any subdivision thereof—they are not, for example, what happens when a skillful wish evades a censor—but simply as ways of representing relationships among responses and controlling variables. I have elsewhere tried to demonstrate this by restating the Freudian mechanisms without reference to Freudian theory (*4*).

5) Since Freud never developed a clear conception of the behavior of the organism and never approached many of the scientific problems peculiar to that subject matter, it is not surprising that he misinterpreted the nature of the observation of one's own behavior. This is admittedly a delicate subject, which presents problems which no one, perhaps, has adequately solved. But the act of self-observation can be represented within the framework of physical science. This involves questioning the reality of sensations, ideas, feelings, and other states of consciousness which many people regard as among the most immediate experiences of their life. Freud himself prepared us for this change. There is, perhaps, no experience more powerful than that which the mystic reports of his awareness of the presence of God. The psychoanalyst explains this in other ways. He himself, however, may insist upon the reality of certain experiences that others wish to question. There are other ways of describing what is actually seen or felt under such circumstances.

Each of us is in particularly close contact with a small part of the universe enclosed within his own skin. Under certain limited circumstances, we may come to react to that part of the universe in unusual ways. But it does not follow that that particular part has any special physical or nonphysical properties or that our observations of it differ in any fundamental respect from our observations of the rest of the world. I have tried to show elsewhere (*4*) how self-knowledge of this sort arises and why it is likely to be subject to limitations that are troublesome from the point of view of physical science. Freud's representations of these events was a particular personal contribution influenced by his own cultural history. It is possible that science can now move on to a different description of them. If it is impossible to be wholly nonmetaphorical, at least we may improve upon our metaphors.

The crucial issue here is the Freudian distinction between the conscious and unconscious mind. Freud's contribution has been widely misunderstood. The important point was not that the individual was often unable to describe important aspects of his own behavior or identify important causal relationships but that his ability to describe them was irrelevant to the occurrence of the behavior or to the effectiveness of the causes. We begin by attributing the behavior of the individual to events in his genetic and environmental history. We then note that because of certain cultural practices, the individual may come to describe some of that behavior and some of those causal relationships. We may say that he is conscious of the parts

he can describe and unconscious of the rest. But the act of self-description, as of self-observation, plays no part in the determination of action. It is superimposed upon behavior. Freud's argument that we need not be aware of important causes of conduct leads naturally to the broader conclusion that awareness of cause has nothing to do with causal effectiveness.

In addition to these specific consequences of Freud's mental apparatus in obscuring important details among the variables of which human behavior is a function and in leading to the neglect of important problems in the analysis of behavior as a primary datum, we have to note the most unfortunate effect of all. Freud's methodological strategy has prevented the incorporation of psychoanalysis into the body of science proper. It was inherent in the nature of such an explanatory system that its key entities would be unquantifiable in the sense in which entities in science are generally quantifiable, but the spatial and temporal dimensions of these entities have caused other kinds of trouble.

One can sense a certain embarrassment among psychoanalytic writers with respect to the primary entities of the mental apparatus. There is a predilection for terms that avoid the embarrassing question of the spatial dimensions, physical or otherwise, of terms at the primary level. Although it is occasionally necessary to refer to mental events and their qualities and to states of consciousness, the analyst usually moves on in some haste to less committal terms such as *forces, processes, organizations, tensions, systems,* and *mechanisms.* But all these imply terms at a lower level. The notion of a conscious or unconscious "force" may be a useful metaphor, but if this is analogous to force in physics, what is the analogous mass that is analogously accelerated? Human behavior is in a state of flux and undergoing changes that we call "processes," but what is changing in what direction when we speak of, for example, an affective process? Psychological "organizations," "mental systems," "motivational interaction"—these all imply arrangements or relationships among *things,* but what are the things so related or arranged? Until this question has been answered the problem of the dimensions of the mental apparatus can scarcely be approached. It is not likely that the problem can be solved by working out independent units appropriate to the mental apparatus, although it has been proposed to undertake such a step in attempting to place psychoanalysis on a scientific footing.

Before one attempts to work out units of transference or scales of anxiety, or systems of mensuration appropriate to the regions of consciousness, it is worth asking whether there is not an alternative program for a *rapprochement* with physical science that would make such a task unnecessary. Freud could hope for an eventual union with physics or physiology only through the discovery of neurological mechanisms that would be the analogs of, or possibly only other aspects of, the features of his mental apparatus. Since this depended upon the prosecution of a science of neurology far beyond its current state of knowledge, it was not an attractive future. Freud appears never to have considered the possibility of bringing the concepts and theories of a psychological science into contact with the rest of physical and biological science by the simple expedient of an operational definition of terms. This would have placed the mental apparatus in jeopardy as a life goal, but it would have brought him back to the observable, manipulable, and preeminently physical variables with which he was in the last analysis dealing.

References and Notes

1. E. Jones, *Life and Work of Sigmund Freud* (Basic Books, New York, 1953), vol. 1.
2. E. Frenkel-Brunswik, p. 293 of this issue.
3. Although it was Freud himself who taught us to doubt the face value of introspection, he appears to have been responsible for the view that another sort of direct experience is required if certain activities in the mental apparatus are to be comprehended. Such a requirement is implied in the modern assertion that only those who have been psychoanalyzed can fully understand the meaning of transference or the release of a repressed fear.
4. B. F. Skinner, *Science and Human Behavior* (Macmillan, New York, 1953).

St. Jerome, Scholar, Translator, Archeologist

St. Jerome (see cover), born at Strido in Dalmatia, got his education in Rome, where he studied grammar, rhetoric, Latin poetry, and philosophy and spent his Sundays in the catacombs discovering the graves of martyrs and deciphering inscriptions. Afterward he traveled and studied in Greece, Gaul, and Asia Minor. In Chalcis he discovered manuscripts and began to study Hebrew. He entered the Arian controversy with vigor, learned Greek, and was called to Rome in 382 by Pope Damasus, who later suggested that he revise the "Old Latin" translation of the Bible. The result after many years was his great work the Vulgate.

On the Scientific Status of Psychoanalysis

JEROME RICHFIELD

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FROM the time of the well-known polemic by Knight Dunlap in 1920 to the recent publication of the Hixon lectures on *Psychoanalysis as Science* (*1*), much energy has been expended, and perhaps dissipated, in an effort to determine the so-called "scientific status of psychoanalysis." This paper is primarily an attempt at a brief consideration of some of the problems underlying such efforts. I wish to suggest that the questions and statements ordinarily involved in discussions of the relationship between psychoanalysis and science are arbitrary, confused, and inadequate. I hope to show that the problem of the scientific status of psychoanalysis needs restatement.

Discussions of psychoanalysis as a science differ in at least one significant respect from discussions of, say, physics as a science. When psychoanalysis is discussed in this connection, the questions asked are aimed at a supposed need of determining whether psychoanalysis is fundamentally and genuinely scientific, but when physics is discussed the questions are aimed at discovering the nature of science itself. Thus, whether or not psychoanalysis is deterministic has been considered a factor in evaluating its status as a science. Yet in quantum physics, the limitations set by Heisenberg's uncertainty relationship on any strict application of the principle of causality has resulted in the judgment that *science* is no longer deterministic (*2*). There is no special difficulty in this particular difference between the two fields except for its effect upon the philosophic consideration of questions about the scientific status of psychoanalysis.

In our present culture, such questions are important because of the eulogistic function, or emotive significance, of the term *science*. It seems to be assumed that to question the *scientific* status of an activity is equivalent to asking whether that activity is desirable, reliable, and valid.

The disinterested opponents of psychoanalysis usually find the ground for their repudiations in a discrepancy between some aspect of psychoanalysis and some concept of science. The scientific character of psychoanalysis has been impugned on the ground that, as a dynamic theory of human behavior, it is deterministic, although man is not a machine. And psychoanalysis has also been assailed as unscientific because it formulates motive explanations instead of restricting itself to the causal type of statement attributed to the orthodox determinism of neurophysiology and biochemistry.

While one commentator claims that there are only two accounts of psychosomatic sequences that have scientific status, because these alone reveal the attention to mechanism that he requires of any theory (*3*), another describes the vastly diminished importance of mechanical explanation in contemporary science and holds that such attempts at explanation are a Victorian remnant which recent events in physics have shown to be impossible or undesirable (*4*).

Most critics have agreed very little on whether the chief difficulty with the scientific character of psychoanalysis should be said to be its limitations in the way of controlled experimentation, its relative lack of predictive ability, its difficulties in the way of quantification and measurement, its extraordinary profusion of variables, the complexity of its terminological innovations, its paucity of statistical correlations, the existence of broad theoretical schisms among its practitioners, the subjectivity of many of its data, the metaphorical character of its basic terms, the role of the investigator's personality as an intervening variable, the absence of operational definitions, or, in general, the failure of psychoanalysis to resemble modern physics in significant respects.

It should not be at all surprising that this bewildering array of diverse criterions results from questions about "the scientific status" of any field. Although these questions are regularly phrased in familiar terms, it does not seem to have been sufficiently appreciated that the common unexplicated use of a term presupposes a common acceptance of its meaning. *Science* and *psychoanalysis* are terms of varying applicability. Neither has both a fixed

and a definite denotation. Both terms are to be found in controversial uses. Many statements that involve the terms *science* and *the scientific method* have no clear sense, no unambiguous use. And since the term *scientific* has a mixed descriptive and evaluative, or prescriptive, function, the ostensibly simple questions about the relationship of psychoanalysis and science take for granted a variety of preconceptions that need to be justified.

Questions about the nature of science are not scientific questions; they are not like those concerned with the nature of zebras, falling bodies, electrons, and physiological functions. They are questions that involve a technical philosophic analysis in terms of some combination of sociological, semantic, ethical, logical, epistemological, and historical facts and values.

Perhaps it is not the privilege or the responsibility of the philosopher to make very many of the prescriptions that would be entailed by a critical use of the term *scientific*. But it is a necessary and characteristic philosophic activity to disclose the existence and significance of these prescriptions through an analysis of the actual uses of the term.

First, it should be recognized that determinations of scientific status always involve the use of certain arbitrary stipulations in the way of meaning. This may be clearly seen in a consideration of a few of the historical objections that have been made against psychoanalysis.

One old and persistent charge against psychoanalysis compares it with mysticism (*5*). The argument here depends upon the proposition that knowledge is of two kinds, perceptual and inferential. Mysticism is defined as the belief in the cognitive function of some third kind of experience which is rejected by the nonmystic as being simply emotional. When it is noted that many psychoanalytic data are derived from certain empathic processes, such as the so-called "transference relationship" between patient and analyst, it is argued that the result can be only the mystical third kind of experience and cannot be taken account of by what is deemed "genuine science."

The statement that knowledge is either perceptual or inferential must itself be either a proposition of logic or an empirical finding. In the first case, the denial of cognitive significance to any emotional process would result from a trivial tautology. And whether or not the determination to use the word *knowledge* in this way is conventionalized, it is inconsistent with the professed aims of scientists, since it logically precludes certain factual investigations.

If the statement is intended to describe a factual discovery and is not an *a priori* legislation, then psychoanalysis cannot be judged unscientific merely on the ground that some of its statements are functions of certain affective relationships. The psychoanalyst might explain the so-called "empathic understanding" as a kind of ordinary perception, or he might claim to have, in fact, some third kind of knowledge that is neither perceptual nor inferential. In either case, the usefulness of the empathic technique does not depend upon its explanation. The question is whether testable hypotheses are obtainable from the characteristic activities of the psychoanalyst, and not whether the psychoanalyst, like the mystic, assigns a cognitive function to various emotional states.

Some traditional objections to psychoanalysis, although not themselves suffering from faulty logical or semantic formulation, depend upon certain unfortunate ways in which some psychoanalysts express their hypotheses. It has been said repeatedly that various psychoanalytic hypotheses violate a fundamental canon of scientific method to the effect that statements must be expressed in a manner that allows for the possibility of disproof. Here the rule is entirely acceptable. We cannot be said to be making a significant assertion if its falsity is inconceivable. Do psychoanalysts commit this error? It does appear from some psychoanalytic statements that human behavior is divided between pathological excesses and their reaction formations. It has been shown that psychoanalysts will support the concept of an anal character by citing studies "which found a higher frequency of reported stinginess, and *also of extravagance* among adults who confessed to a recall of some form of anal eroticism"(*6*).

Critics have claimed that these examples of psychoanalytic statements violate our methodological canon in arguing "that *opposite* traits reveal the *same* underlying condition." Despite the apparent plausibility of this important criticism, it can readily be shown that the objection involved does not apply. The characteristic psychoanalytic hypotheses that concern us here are highly elliptical statements concealing much theoretical complexity. We should not understand the psychoanalyst to be asserting simply that the concept of an anal character implies stinginess in one case and extravagance in another. What is intended is the statement that an anal character, together with certain variables, implies stinginess in one case, while an anal character with *different* variables in another instance implies extravagance.

In some cases, the so-called "opposite" traits, which are held to reveal "the same underlying condition," are not real contraries. And their logical incompatibility in the same hypothesis is apparent

only. The psychoanalyst will relate both extravagance and stinginess to an anal character by analyzing extravagance into denied stinginess. The difficulty arises when the single term *stingy* is used indifferently in referring to certain modified impulses as well as to various instances of overt behavior. The patient who hoards and the patient who reacts with extravagant behavior to his primary impulse to hoard are not "stingy" in the same sense. This is very often obscured by the psychoanalytic statement that the extravagant patient is "really" stingy.

It is frequently objected that psychoanalysis has not yet exceeded the outmoded level of 19th century biology in its use of the concept of instinct, or instinctual drive, as a basic explanatory mechanism.

There is no question that psychoanalysts use the term *instinct* and its cognates extensively. But let us question this usage to determine whether anymore is ordinarily involved in its psychoanalytic meaning than some unspecified analogic function. Most psychoanalysts do not believe that there are any basic biological forces that manifest themselves in human behavior independently of the influences and conditioning of a variety of environmental factors. They deal with the concept of a kind of mental phenomenon experienced directly as an "urging energy," which, in combination with certain external stimuli, will lower the excitation level of the organism through the discharge of tension.

These urges have some of the characteristics associated with the biological notion of instinct. But they differ in significant respects. The primary urges referred to in psychoanalytic literature are neither biologically inherited nor unalterable in the face of such external stimuli as social, economic, and political factors.

From the point of view of the semanticist, it is unfortunate that psychoanalysts use the word *instinct*. But this does not falsify any psychoanalytic correlations or invalidate the principle of the homeostatic model used in psychodynamic explanation.

We might hope to settle disagreements concerning the scientific status of psychoanalysis if we were to suceed first in producing a sufficiently *persuasive* definition of *science*. Then by examining psychoanalysis, we might note the extent of its conformity to this newly constructed denotation. But this procedure would not preclude the possibility, or even diminish the likelihood, of persistent and warranted dispute.

Suppose the following question arose: How much conformity is necessary before psychoanalysis should be awarded its scientific credentials? To show how this question of the degree of conformity might arise, let us consider briefly the problem of experimentation, an activity closely associated with the present vague notion of science and a characteristic of varied importance in most of the generally accepted special sciences.

Experimentation does take place within psychoanalytic psychiatry but not without certain difficulties and limitations that distinguish it from the experiments of the physicist or the chemist. First, most of the behavioral states involved in psychoanalytic study are not reproducible under laboratory conditions; yet experimentally designed conflict-situations and controlled study of various toxic psychoses can yield some analogous phenomena. The experimental psychoanalyst must make cautious inferences from these.

The psychoanalyst is limited as well by the fact that he himself is inescapably a part of the very subject of his investigations. The personality of the experimenter is one of the significant variables to be controlled. This does not make psychoanalytic experimentation unfeasible. In, principle, at least, the integrity of the experimental subject can be preserved by interpreting all results in light of this factor of counter-transference. The principles involved in the interpretation of the transference are not peculiar to experimentation in psychoanalysis or devised expressly to resolve the experimental problem. They constitute the core of psychoanalytic therapy and are derived from basic psychoanalytic hypotheses.

Finally, in contrast to physics, which is an ahistorical science, psychoanalysis is often concerned with factors that gain their psychological significance at some particular point in the history of a personality. Even if the experimental simulation or reproduction of such a factor were possible, its action upon a different personality, or upon a different phase of the development of the same personality, would very likely be meaningless. It could not, then, be considered a requirement of systematic study.

It seems that there is neither a clearly justifiable basis nor any regularly accepted criterion for attempting to fix "the scientific status of psychoanalysis." Most methodological criticism of psychoanalysis has been a futile attempt to shape it more and more into a form that criticism within physics is incessantly changing. We should be explicit in recognizing that, if the general characteristics of what we now mean by *physics* are to determine the meaning of *science* and, therefore, the *scientific status* of another kind of investigation, discussions about science and psychoanalysis will have another

complexion. For any question of the resemblance between psychoanalysis and physics does not, by itself, seem as important a matter as the present question of whether psychoanalysis is to be termed *scientific*. Thus, if the meaning of *science* is to be purely a pragmatic convention, determined and exclusively modified by the nature and problems of quantum physics, then the question of psychoanalysis as science might be unworthy of our consideration. But if *science* is to mean, among other things, some distinctive ways in which men actually deal with their experiences, then it is the responsibility of the psychoanalyst to play a prominent role in the creation and development of scientific principles. Psychoanalysis cannot passively await the outcome of the convulsions that currently beset the philosophy of science.

The psychoanalyst should apply certain methodological norms and techniques of research when these are compatible with the nature of the facts upon which his work depends. He should not conform to certain procedures simply because they are characteristic of a highly regarded physics or are in some questionable way associated with a vague and arbitrary abstraction like the general notion of "science."

The term *scientific method* is at present clearest and most useful when it is taken as a historical expression, designating the class of different procedures that men in search of systematic factual knowledge have found best suited to the realization of their goal. Any further specification would be groundless and productive of nothing except endless dispute.

References and Notes

1. Stanford Univ. Press, Calif. (1952).
2. See, for example, M. Schlick, *Philosophy of Nature* (Philosophical Library, New York, 1940), p. 70.
3. J. O. Wisdom, *Brit. J. Med. Psychol.* **26** (1953), pt. I, p. 15.
4. M. Johnson, *Science and the Meanings of Truth* (Faber & Faber, London, 1946).
5. See K. Dunlap, *Mysticism, Freudianism and Scientific Psychology* (Mosby, St. Louis, 1920).
6. R. E. L. Faris, *Am. Sociol Rev.* **18**, 438 (1953).

Notes on the Discussion between Frenkel-Brunswik and Skinner

MICHAEL SCRIVEN

Michael Scriven, who was born in England, graduated from the Honours School of Mathematics at the University of Melbourne, and wrote his master's thesis on the limitations of symbolic logic. After two years of research at Oxford University—in philosophical psychology—he joined the faculty at the University of Minnesota, where he now divides his time between the Department of Philosophy and the Minnesota Center for the Philosophy of Science.

A NUMBER of interesting points have been raised by the first two papers in this symposium, but I wish to confine myself to examining one point which I think represents the fundamental issue between them. It is this: Does the theoretical aspect of psychoanalytic discussion contribute anything useful over and above the admittedly important observed correlations (for example, between specified types of trauma and neuroses)?

Now, it might be possible to settle the matter by appealing to the evidence—by showing that diagnoses based on the theory are therapeutically superior to those based only on knowledge of the correlations. But even if this was the appropriate place for such a discussion, most of us realize that it would not be conclusive. Moreover, it would not go to the heart of the matter: for Dr. Skinner disagrees in a fundamental way with the method involved rather than with the actual results of the method. He does not believe that the process of hypothesizing in a new theoretical language is ever fruitful.

It seems to me that there are two weaknesses in this position. First, I would disagree on historical grounds. I believe that the suggestive power of conceptual terms is indispensable to scientific activity; and I do not think that this can be dismissed as a property of an immature science—it seems to me to be what distinguishes a science from a summary. In fact, it seems quite evident to me that many of the great steps in the history of science have not only followed the sudden perception of familiar

events in a new way, according to a new model—in short, as a result of a new hypothesis—but that they could not have been made otherwise, for example, Lavoisier with the theory of combustion, Kepler with the planetary motions, Guericke and the spring of the air, and Freud and the unconscious wish—these were new ways of interpreting correlations, not new correlations. But, of course, they immediately *led* to new correlations, which makes the Skinnerian interpretation of history more plausible at first sight than a precise chronological account leaves it. Further consideration of the historical point leads one to look for a reason for the importance of hypothetical concepts, which forms the second and more important of my two arguments against Skinner. It is a logical point rather than an empirical one.

I do not think that there is a fundamental difference between the observation terms and the theoretical terms of science, even at a given moment. There are *important* differences, indeed, between terms such as "length" or "response frequency," on the one hand, and others such as "entropy" or "intelligence." But it is a mistake to think that these differences correspond to the difference between theory-neutral and theory-contaminated terms. For observation terms are also embedded in a theory, albeit a very well-established one, and every now and again we are awakened by the discovery that this theory is not inviolable and that we have to change our concept of let us say, length in a fundamental way, perhaps by introducing a reference to the velocity of the measured object. Conversely, although we may observe entropy changes or intelligence levels in a different way from length and response frequency—a way that makes people want to say that we really *infer* them from other observations—this is only half the truth, for we do *observe* that a man is intelligent or introverted or unconsciously motivated. So the distinction between psychoanalytic explanation using Freudian language and quoting the correlations between behavior observations cannot be regarded as very fundamental—although I do not wish to deny that insofar as Freudian language is ambiguous, vague, or even inconsistent in application, it is less satisfactory in these respects than the comparatively simple (and less potentially fruitful) language of overt behavioral observations.

I find it difficult to say anything much more precise than this on the issue. Skinner takes a rather elusive position; once we have conceded to him that psychoanalytic theory is far from precise, containing perhaps very few concepts that are pragmatically valuable, and he has agreed with us (as he has done elsewhere) that "botanizing" is not enough in behavior science—that more is involved than the comparatively simple sort of correlations that form the basis of a classificatory system—then we find it hard to locate an unambiguous position for him between these alternatives. Perhaps we can best understand his attitude in terms of his own practice. He is not, in my understanding, primarily interested in the explanation of behavior but rather in providing the descriptive laws that form an important part of explanations. To bring an old and rather worn distinction into service, Skinner's interest appears to be in answering the "how" rather than the "why" about behavior. I do not wish to claim here that there is an absolutely fundamental difference between the answers to these questions, but there is at least a difference of level in the explanation, and this amounts to saying that the generalizations involved in the answer to the "why" question can be used to deduce more results than those connected with the "how" question. The answer to "why" requires at least a law of sufficient generality to subsume under it the law that describes "how." Now the area in which Skinner works, investigating one particular type of response under a few particular types of motivation and conditioning in a few organisms, is certainly an area where the "how" question needs answering and it may be done—given experimental ingenuity and perseverance—within a very simple theoretical structure. Almost, in fact, within the theoretical structure of ordinary observation-of-behavior language. But even in such an area there can be people who try, not to find, but to explain once found, the form of the regularities. Their quest is surely legitimate although certainly sometimes premature. And so the process continues, perhaps not unilaterally, perhaps establishing linkages in many different directions, toward engineering and algebra as well as toward neurophysiology. But this process cannot continue without bridging the gaps between the different levels of law, between the incommensurate units and models of different subjects; and one cannot bridge these gaps without hypotheses, concepts, and theoretical terms of which the Freudian vocabulary affords an interesting, though imperfect, example.

Chippewa Sacred Songs in Religious Metamorphosis

GERTRUDE P. KURATH

Mrs. Kurath, who was trained in dance, music, and theater technique and at Bryn Mawr and the Yale School of Drama, had a career as a concert performer, teacher, and producer before engaging in what has proved to be pioneer work in musicology and choreography. Her field trips and publications have ranged from the Mexican Yaqui and Otomí to the Great Lakes Iroquois and Algonquians. Mrs. Kurath contributed the dance entries to the Dictionary of Folklore, *is serving as dance consultant to* Webster's New International Dictionary, *and is assisting in a University of Michigan broadcast series on Michigan Indians. She lives in Ann Arbor where she is president of the Michigan Folklore Society.*

THE search for music indigenous to this continent brings constant surprises, even close to home in the heart of the Great Lakes area. The Chippewa (*1*), Ottawa, and Potawatomi Indians of Michigan have offered little attraction to the student of exotic cultures, because in daily life they live and dress like their white neighbors. At shows they don the feather bonnets of their Great Plains archenemies of yore. At religious meetings they sing the hymns of their usurpers. Yet their cultural and religious transformation, instead of shriveling their musical propensities, has produced a two-way stretch. It has crossed two traditions, each with a complex past, two traditions that appear incompatible.

What artistic, religious, or historical factors facilitated the final acceptance of an alien style from unwanted intruders? This problem varies in different parts of Michigan, even within the Lower Peninsula with its Catholics at Little Traverse Bay and its Methodists near Charlevoix, Athens, and Mount Pleasant. I shall investigate (*2*) the answer only for the Methodist Algonquians at Black River Reserve near Mount Pleasant, where most Indians know hymns in their native language, but few remember their ancestral songs.

The Songs

The old and new songs, native and Christian, now pertain to two types of gatherings—the show, or "powwow," and the religious revival. The native ceremony has become a secular event that attracts a white audience, but which also draws together tribal members. During the weekend of 4 July the Mount Pleasant Indians present a pageant under the direction of a Chippewa named Wassěškom, alias Eli Thomas. For a weekend in mid-August scattered Algonquians assemble from all sections of the Lower Peninsula and camp in Charlton Park near Hastings. On Sunday morning they attend a service on an outdoor platform. At night they perform by a campfire for a semicircle of white spectators. Eli Thomas and Whitney Albert of Mikado take turns at drumming and singing; young men dance solos; the entire group, male and female, joins in a welcome dance and an adoption dance. Late at night, after the departure of the audience, the Indians gather informally around the fire, and at that time they sing their best songs and tell their best stories.

During the week before Labor Day a wooded knoll on Black River Reserve is the site of an entirely different kind of *wikundawin,* a Methodist camp meeting. Algonquians assemble not only from Michigan but also from adjacent Ontario. Three hundred come from local farms; 700 arrive for a day by chartered bus or pitch their tents in a circle for the duration of the meeting. Preachers, self-appointed evangelists, and laymen worship in an open wooden tabernacle with plank benches facing a crude raised platform. Most of the time the entire congregation sings in Chippewa or English, unaccompanied or to Lucy Pelcher's piano. Often the music forms an obligato to ecstatic shouting and praying as men and women cluster about a kneeling convert, "Jesus, have mercy on this miserable sinner! *Shawendagoze!*" Witty bilingual sermons and Chippewa benedictions informally unite the Indian preachers and the congregation; a Sunday afternoon communion service more formally honors

the two white ministers. On Sunday, the soloists sing their best, two steel guitars and an accordian harmonize the unison singing of a children's chorus and the four-part hymns of adult choruses, and Ford Soney plays the piano for the expert Walpole Island choir.

Neither Indian dancing nor square dancing is permissible at these meetings. But the conversions entail impromptu rhythmic activity, walking, clapping, and swaying in a complex group counterpoint. The grand finale of the last Labor Day service is the "Doxology," sung in a huge circle with joined hands.

Three styles. From recordings made on these occasions, a few examples have been selected to represent the old, the transitional, and the new song types. The "Hoot Owl Song" (Fig. 1), a favorite among Michigan Indians, typifies the aboriginal style (*3*). It features in all shows. In more intimate programs, Eli Thomas drums for the "Grandfather Song," a transitional hymn, dating back 130 years, to a momentous period in Michigan Indian history (Fig. 2). The chorale in Fig. 3 was composed by Lowell Mason and published in 1835 as "Uxbridge" (*4*). It was sung in parts by the revival members and accompanied on the piano.

Harmonization provides the most obvious contrast to the drumming and single melodic line of the native and transitional songs, but it is not the only contrast. The legato style of hymn singing differs from the staccato, emphatic rendering of the native songs. Like the "Hoot Owl Song," 80 percent of the Michigan Indian songs stress the interval of the fourth, in a descending scale of 54 21 (fifth, fourth, second, and main tone). Thus, they lack the third, the central note of the western European triad. On the other hand, the hymns emphasize the third in a complete diatonic, or seven-tone, scale. The "Grandfather Song" also runs up and down this conventional scale, but with a flat "leading tone," or seventh.

All three songs are constructed on the principle of a sequence, a thematic recurrence on different levels, constantly descending in native songs but ascending and descending in the hymns (*5*). The predominant rhythmic patterns of these themes, the simple combinations of eighth and quarter notes, suggests one point of contact, despite the slower tempo of the hymns. The even percussion backgrounds are also similar, despite the ocean separating the instruments. While the percussion proceeds steadily to the end, the melodies are divided into phrases concluding on a long note. In the native songs these phrases are of uneven lengths, in the "Grandfather Song" they shift accents, and in the hymns they parcel off into four square, equal groups.

Texts. In all three song types each phrase fits to a line of text. The "Hoot Owl Song" says,

kukuku ƚ u	nįŋgǫsá (*6*)
hoot owl	I'm afraid (of)

Eli Thomas explains, "Long time ago when I was a small boy I was taught after the sun went down

Fig. 1. The "Hoot Owl Song" is one of most popular and typical of Michigan Indian native songs. Drum accompaniment.

Fig. 2. The "Grandfather Song" combines Indian rhythm and drum background with the European scale, Ojibway words with Christian thought.

I must be quiet. They said to me, 'Don't do that. Owl going to come and get you'." Ritual associations have vanished into a misty past.

But the words to the other two songs are fully understood and well documented. Figure 2 uses the first two verses; Fig. 3 uses all six verses of "Jesus ishpeming" which were translated by Thomas as follows, with the spoken, not the sung, accents:

1) jízas íšpimiŋ káyežad
Jesus heaven he went
mísə yawæpénimoyan
I'm the one depending on Him
niwabindán gá:niyežad
I've seen it (where) He went
káye ningéniyežayad
that's me going the same way

2) gá:nežawá:d ka:nibwə́kadik
wherever went the prophets (saints, wise)
mi:kọ́s kuyə́k ænimənik
the road is made straight (goes *way* straight up)
káye ninkemá:didóyan
I'm going to follow (that road)
mi:kọ́s kǽče šawéndawik
the road (is) happy (blessed)

3) míso ˈo ká:miné:žiyan
that's the one that was lacking me
káyunje kidimá:giša:n
poor weak
ki:kéniməsiwók ma:bə́
before I knew Him (that one)
kiːžé mənedó owisá:n
Great Spirit His Son

4) kæwezdə́š ka:gi:bíŋwayọn
the longer I was (stayed) blind
æšškom ningí:kidimá:gis
for that long time I was poor
nəmóˈædəš nəbiji:nəg
just a little while ago
ningí:bi:žawénimigo
I had a blessing (I got saved)

5) níwə wižeńdəm bižáyọn
I'm glad that you came
čiwədápiniyọn dəškín
that you take me
wá:sə níndəpəgidá:nən
away I throw
níŋgagi:bá:dišæwinan
my sins (I'm throwing my sins *away*; gi:bá:dis means foolish)

6) kidimá:gižijíg mæže
all are poor, every one
níŋgəbaba:wíndəməwok
I'm going to tell them (the sinners all around)
čimikəwa:wad kosená:n
so they can find God (our Father)
níŋəwižinúygməwok
I'm going to point it at them (I'm going to point to my Savior for my brother sinners)

Both songs, in fact, many of the hymns, add the following verses:

má:mayu: wamá:dama:bə
give Him the high honor (thank Him, praise means to thank)
wǽnji šawéndagəziyọ
that's the one that blesses, that creates (blessing flows, you can't run out of it)
wé:yosimint wékwisimint
the Father the Son
káye panižíd oǰičág
also the Holy Ghost (the awful clean spirit)

Eli Thomas knows these words by heart. Those who do not can read them in *Ojebway Nuhguhmonun,* a collection of Wesleyan hymns translated by Peter Jones, published in 1847 and again in 1854 (7). The first six verses constitute "Hymn 50," or "*Nagamowin* 50," with the English original on the page to the left and the Chippewa, in nonphonetic orthography, to the right. Eli Thomas says of the translation, "We say it more plainer. We don't beat around the bush." One is inclined to agree with this statement on reading the English poetry by the Moravian Englishman, John Cennik (1718–1755). Thus, the first verse goes

Jesus, my all, to heaven is gone;
He whom I fix my hopes upon:
His track I see, and I'll pursue
The narrow way, till him I view.

The extra concluding verse is Peter Jones' "*Nagamowin* 130," better known as the "Doxology," the 17th century creation of Bishop Thomas Ken (1651–1710).

Fig. 3. This melody for "Jesus ishpiming" fits into hymn style by its tonality, harmonies, and piano accompaniment.

Praise God, from whom all blessings flow,
Praise God, all creatures here below;
Praise him above, ye heavenly host;
Praise Father, Son, and Holy Ghost!

These texts were all published in John Wesley's collection of psalms and hymns in London in 1738, and they were sent to America on printed sheets in 1784 (*8*). In accordance with the practice prevalent until 1855, tunes and texts were issued in separate books, and their combination was left to the discretion of the song leader. This custom survives among the Indians. They sing Bishop Ken's words as a conclusion to various hymns as well as to the famous melody composed in 1550 by Louis Bourgeois, the "Old Hundredth," originally fitted to Psalm 100, "A Psalm of Praise." The text of "Jesus ishpiming" can also fit various eight-syllable tunes with some juggling of accents. It was recorded to a four-syllable tune. Evidently the most popular hymns owe their prestige not only to their melodic qualities but also to the message inherent in the texts.

Aboriginal Causes of Acceptance

These texts may offer a clue to the native experiences and concepts, as expressed in native terms and as equated with English concepts and terms. "Jesus ishpiming" reiterates confidence in salvation and in relief from misery by following the divine intercessor to a better existence. This ideology finds no correspondence in the current native songs, which tell of animals, war, and conviviality. But similar noble invocations were contained in Wabano (*9*, pp 247–248) and Midewiwin (*10*) songs recorded 50 years ago. And various aspects of Algonquian faith and customs could readily be reinterpreted.

Beliefs. Half a dozen expressions from "Jesus ishpiming" illustrate ideological correspondences.

Jesus in invoked by His Christian name, in English pronunciation. But He is also addressed as *Kiže mənedo owisan* (Son of God) or as *Wekwisimint*. His role as intercessor is well understood, since the aboriginal approaches to an all-pervading deity were negotiated through spiritual intercessors, through natural forces invoked by the whole tribe, or through individual guardian spirits received at adolescence in a vision. Such beliefs persisted in reports from the 17th century (*11*, pp. 10, 84–85, 265–267) through the 19th century (*12*, pp. 5, 64). They account for the origin of the Mide society, in one version through the culture hero Manabosho (*12*, p. 67), in another version through the East Manido, Wabanunk'daci' manido (*10*, pp. 21–23). Self-sacrifice can be understood from Indian legend and conduct, such as in the voluntary death of Jibiabos in order to lead the way to the spirit land and in the death of Chief Bianswah for his son.

Išpimiŋ literally means high in the air. The Chippewa language has no word for heaven and no concept of a paradise in the sky. But the historic Indians believed in the immortality of the soul, *jibam*, and in a spirit land to the West, *abəŋgišimok*. The road to this land is arduous, judging by the story by Eli Thomas and by many writers, such as Warren (*12*, p. 49) and Jones (*13*, p. 103).

Mi:kos kuyək ænimənik, the path straight up, was formerly neither straight nor up. The soul met many obstacles. A spoonful of a huge *odæmin*—"heart berry" or strawberry—gave the soul strength to cross a treacherous river on a wriggling bridge and to reach a blessed land with all the pleasant things of life but none of its miseries (*14*). Thomas regards this land as a sort of first paradise, or purgatory, and thus conciliates the two faiths. Hence, this is also the blessed road, *mi:kos kæče šawendawik*.

Kidima:gis expresses the poverty and helplessness that have confronted the Indian as a grim reality. In their aboriginal state, wintry privations hounded them. In the early 19th century, the gadgets of civilization proved a doubtful blessing. They struggle against cold and hunger, and are very grateful for the least relief. *Kitte-mau-giz-ze Sho-wain-e-min*, is their common expression to an agent —"I am poor, show me pity" (or rather, "charity me") (*15*, p. 472). These supplications required little metamorphosis in transference to a new ideology. "Here it means I wasn't saved," said Thomas, "but we have poor people in town. You can use it that way too."

Šawenda stands for all happy states and activities that solve *kidima:giži*. It recurs in hymns, in prayers, in ejaculations, and in daily life. A blessing is *šawendajigewin*; the one who blesses is *wænji šawendagəžiyo*. Eli Thomas says, *"Kižawendagude* comes from Jesus' sacrifice, for he saved us, so you got the blessing, your sin is forgiven."

Mənido, the term for spirit, is not very ancient according to Schoolcraft. "It is, properly speaking, not the term for God, or Jehovah, but rather a generic term for spiritual agency in their mythology" (*15*, pp. 55–56). The multiplicity of these spirits and their function in nature were remarked by French Jesuit missionaries around 1700 (*11*, pp. 287–288); they were identified as helpful or harmful. Although Baron Lahontan spoke of prayers to Kitchi Manitou (*11*, p. 291), the concept of a pervading power remained a mystery. Even in the 19th century, when Christianity had modified the ancient beliefs (*16*), the Indian supreme God and his name remained ambiguous. Both the concept and the name have a double explanation—*Keche manedo*, or great spirit, and *Kesha manedo*, or benevolent spirit (*17*). It is still pronounced in two ways. Other names have been adapted to hymnody—*Nosæ* or *Kosena:n* for Our Father, *Tebenege* for ruler and lord, and *Ogima* for chief, in the doxology "Weyosimint."

Mənido did not serve for Holy Ghost, especially since there seems to have been no term for holy. So they made the Holy Ghost superlatively clean, *pa:nizid*, and gave Him another name for ghost, *jičag*. A similar predicament for angels was solved in two ways—*išpimiŋ, žewis*

anišinabek (heaven its Indians) and *anže:niyok* directly from the English.

The imagery involved in these spiritual agents was an elusive subject, just as it is today. But the concepts produced concrete ritualistic procedures. A mixed-blood missionary said of them, "In illustrating the nature of the great atonement to my pagan countrymen, I have often made reference to their own sacrifices (*13*, p. 96).

Tribal sacrifices. To be exact, these sacrifices mean offerings by invocations. Offerings of tobacco, dogs, and furs, and later on offerings of firewater, pervaded all activities, private or tribal; offerings were made privately to the guardian spirit, and publicly to the sun or a member of the spirit pantheon. The tribal festivals anticipated the camp meeting of today, for they were social as well as religious. They ushered in a time of rejoicing—the summer and the end of the winter's hardships and starvation; they gave thanks for harvests and beasts, and they petitioned future bounty. Indian bands came from near and far to camp on open grounds, to feast, and to celebrate the exclusive rituals of the Wabano and Mide societies.

These gatherings persisted into the 19th century, as did also the Painted Pole Feast called *Sahsahgewejegun*—the spreading out to view the desires of the supplicant by tying offerings to the top of a painted pole that was elevated (*13*, p. 97). The focal Mide pole, as well as the individual Manito pole, carried similar offerings, against sickness. Near Green Bay Father Marquette noticed a pole with a crossbar near the top "which these good people had offered to the Great Manitou . . . to thank him for having had pity on Them during the winter . . ." (*11*, p. 215). The Indians of today speak of a sacrifice as *sasagiweǰigæ*. The saved Indians, "the G.I.'s of the Cross," continue the custom of summer meetings, traveling from one community to another.

Music and dance. Singing, drumming, and dancing were the focal activities of the tribal festivals. Music was the prerogative of trained male specialists, but women as well as men could join in the preparations and in the dances. Although the women restrained themselves to a quiet side step, the men worked themselves into a frenzy of stamping and shaking. In 17th century outdoor offerings to the sun, children also made a ring around the fire.

> . . . and the Warriors Dance and Sing around them till the whole is Burnt and Consumed, while the Old Men make their harangues address'd to the Kitchi Manitou, and present him from time to time with Pipes of Tobacco Lighted at the Sun. . . . The Women likewise made Adresses to him (*11*, p. 291).
>
> . . . There are four or five young men who sing, and keep time by beating the drum and the sysyquoy (rattle), while the women dance and do not miss a step. . . . Often the old men dance the midelinne (midewin); they look like a band of sorcerers (*11*, p. 270).

Other festivals prepared for war, celebrated victory, or dispatched the souls of the dead, with doleful songs by relatives and hired singers, with contests, races, and feasting. In their special tabernacles, the Mide priests sang their dancing songs, *nimiwug*, while they circled the lodge in a sunwise direction, a leader carrying the Mide pole. Private all-night singing also preceded feasts and hunting expeditions.

Singing, processions, and dancing lasted through the 19th century and into the 20th century. However, around 1900, Frederick Barton, the first and only musical student of this area, found old customs merged with Christianity. His collections from Garden River Reserve not only included some Wabano songs and a number of animal and convivial songs in native style, but they also included half-breed compositions and hymns for wakes and social gatherings, with the inevitable climactic "Doxology" (*9*, p. 236). With the exception of the Wabano songs, the same types persevere. And the ancient all-inclusive participation continues. Children as well as adults sing. Women as well as men offer "Harangues." Only the dance no longer addresses the Great Spirit.

Dynamic Causes of Acceptance

Thus, aboriginal analogies gradually adapted themselves to Jesuit teachings and changed in the course of two centuries. Religious factors combined with other events to stimulate the mass conversions that started in 1823 at the time of the birth of the "Grandfather Song." These factors were *kidima:gizi* of economic conditions and *šawendaǰigewin* preached by Chief Kahkawekonaby, alias Peter Jones.

Economic conditions in 1823. "Previous to the year 1823, at which time I was converted to Christianity, the Chippeway and indeed all the tribes were in a most degraded state; they were pagans, idolaters, superstitious, drunken, filthy, and ignorant" (*13*, p. 236). This statement by Peter Jones about his own people represents the opinion of other writers of that time. After a brief golden era of friendly contacts with French travelers and selfless priests, the fur trade brought wealth to the Astors, but only whiskey, disease, destitution, and destruction of game to the Indian. Unscrupulous land transferences to British and American settlers (*18*) discouraged the pursuit of agriculture and antagonized the Indians against white advances. Yet

the native religion offered no way out of this plight. No prophet arose to fuse the best in the old and new faiths and to ban liquor, as did Handsome Lake for the Iroquois. The Mide religion, which, in its heyday, had taught a good, honest life and sobriety, had degenerated. The multitude of Christian sects added to the Indian's confusion.

A leader on the happy road. But, in the meantime, interracial contacts had brought forth tribal leaders who stood midway between the two faiths, namely, an increasing number of intelligent halfbreeds. One of these, Peter Jones, was raised by his Mississauga mother in pagan practices, but as the son of a Welsh surveyor, he was baptized in the Six Nations Mohawk Episcopal church. After repeated failures in attaining a vision by traditional fastings, he achieved this end, in another way, during a Methodist camp meeting at Ancaster, Ontario.

> In the afternoon of this day (June 3, 1823) my sorrow and anguish of soul greatly increased, and I felt as if I should sink down into hell for my sins; which I now saw were many, and exceedingly offensive to the Great Spirit. [But] At the dawning of the day (June 4) I was enabled to cast myself wholly upon the Lord, and when I received Him unspeakable joy filled my heart (*13*, pp. 7–9).

After an English preparation for the ministry, he devoted his life to the conversion of his tribesmen in 17 Ontario missions, including his own River Credit Reserve. His forceful personality and his blended point of view, symbolized in the pattern of his conversion, accounted for his success.

Process of Acceptance

Although his followers agreed that Christianity had lifted them out of the miry clay, the old shamans noticed that the white man would, after coming out of his praying-house, continue to drink, fight, and cheat; and some of the Indians clung to their old practices until well into the 20th century. However, the Indians who followed Peter Jones evidently did find some security, and they were "sober and comparatively clean and industrious" (*13*, pp. 7–9). They enjoyed the music to which he assigned an important role. In 1843 he reported that the converts "have made some proficiency in singing, are tolerably well acquainted with the rules of sacred harmony, and have a hymn-book translated into their own language which is in constant use," namely, his translation (*13*, p. 237). His influence gradually reached Michigan. In 1855 the establishment of the Black River Reserve drew Indians from the Bay City, Saginaw, and Sarnia districts and brought Methodist hymnody with the immigration.

Metamorphosis has been continuous. At first transitional hymns bridged the gap. For some decades the oldtimers would sing the hymns with an Indian throat and Indian mannerisms (*9*, pp. 136–137), while the younger generation accepted new developments—first the traditional Wesleyan hymns, and later the revival hymns that spread north from Kentucky in the latter part of the 19th century. Today the young people prefer Benson's collection, *Songs of Spiritual Power,* and Billy Graham's *Singing Evangelism,* while their parents cling to the 1910 edition of the *Free Methodist Hymnal* with fine old tunes like "Uxbridge" (*4*, p. 64). Sometimes they set old texts to new tunes and vice versa. Recent verses have been translated into Indian by William Soney of Walpole Island; they appear in his *Ojibway Hymn Book* together with reprints of Peter Jones' most popular paraphrases.

Camp meetings and hymns evidently constitute the reality of the Methodist Algonquian's religious life, often the very essence of his life. Some oldtimers remember native songs and dances as a former part of their worship. They recall legends and retain beliefs in the power of snakes and thunderers; they offer tobacco invocations. In the powwows they attempt to carry on these vestiges of their lore and to emulate the ancient intertribal gathering. Both conservatives and progressives seem content with the adopted forms of worship and with congregational singing in place of native group dancing. Indeed, they are often more fanatical evangelists than their white neighbors who reciprocate by learning Indian ways. Does the revival owe its power to its echoes of ancestral faith and summertime conclaves?

References and Notes

1. *Chippewa* and *Ojibway* are alternate names for the same tribe. The former term enjoys more general usage, whereas the latter term is beginning to prevail in scholarly publications.

2. Acknowledgment is due to the Michigan Academy of Science, hence the American Association for the Advancement of Science, for support of the fieldwork; to the American Philosophical Society for the commission of a manuscript; to Allen P. Britton, director of the Archives of Regional Music at the University of Michigan, for blank tape reels; to Volney Jones and Pierre Dansereau for encouragement; to Kenneth and Evelyn Pike for linguistic assistance; to the muscians, Veronica Britton, Hans David, and Lester McCoy; to Jane Ettawageshik and cooperative Chippewa and Ottawa singers.

3. For comparative purposes the three songs are rendered with the same concluding note and the same note values. Thomas' highest note was really on middle D. The congregation accented and phrased

Fig. 3 as transcribed rather than as printed in the hymn book.

4. T. Hastings and L. Mason, *Spiritual Songs for Social Worship* (Gardiner Tracy, Utica, N. Y., 1835), p. 134.
5. For methods of scale analysis and graphs of descending sequences, see G. Kurath, *Sci. Monthly* **76**, 158 (1953); *U.S. Bur. Am. Ethnol.*, Bull. 156 (1953), pp. 250–253, 263.
6. The minimal phonetic symbols include æ as in bag, ə as in cup, ŋ as in sing, š, č, ž, ǰ as in shine, child, azure, joy. ọ is nasal, : is a vowel prolongation, ʔ is glottal stop. In chippewa, the sounds, accents, and intonation are very elastic, and thereby helped adaptation of translations to hymn tunes.
7. P. Jones (Kahkawekonaby), *Ojebway Nuhguhmonun* (Carlton and Phillips, New York, 1854), pp. 110–113.
8. H. W. Foote, *Three Centuries of American Hymnody* (Harvard Univ. Press, Cambridge, Mass., 1940), pp. 146–147.
9. F. R. Barton, *American Primitive Music* (Moffat, Yard, New York, 1909).
10. F. Densmore, *U.S. Bur. Am. Ethnol.*, Bull. 45 (1910).
11. W. V. Kinietz, *The Indians of the Western Great Lakes, 1650–1760* (Occasional Contrib. Museum Anthrop., Univ. of Michigan, Ann Arbor, 1940).
12. W. W. Warren, *History of the Ojibway Indians* (Minnesota Historical Society, St. Paul, 1885).
13. P. Jones, *History of the Ojibway Indians* (Houlston and Wright, London, 1861).
14. For the diffusion of such beliefs in the Great Lakes and adjacent territory, see G. Kurath, *Midwest Folklore* **4**, 2 (1954).
15. H. R. Schoolcraft, *Personal Memoirs of a Residence of Thirty Years with the Indian Tribes of the American Frontier* A.D. *1812–1842* (Lippincott, Philadelphia, 1851).
16. The effect of Catholicism presents a large and complex subject in its own right.
17. G. Copway, *Traditional History and Characteristic Sketches of the Ojibway Nation* (B. F. Mussey, Boston, 1851), p. 154.
18. The final land transference of southern Michigan territory was affected in 1821 by Governor Cass.

The Truth Is Whole

Use is the aim of Science! give again
A golden sentence to the faithful pen—
Dwell not on parts! for parts contract the mind;
And knowledge still is useless when confined.

The yearning soul, enclosed in narrow bound,
May be ingenious, but is ne'er profound:
Spoiled of its strength, the fettered thought grows tame;
And want of air extinguishes the flame!

And as the sun, beheld in mid-day blaze,
Seems turned to darkness, as we strive to gaze;
So mental vigour, on one object cast,
That object's self becomes obscured at last.

—Elizabeth Barrett Browning

Animal Species That Feed Mankind: The Role of Physiology*

S. A. ASDELL

Dr. Asdell is professor of animal physiology at Cornell University. He was trained at Cambridge University where he majored in physiology and biochemistry. After holding appointments in the Animal Nutrition Institute, Cambridge University, and at the Massey Agricultural College, University of New Zealand, he went to Cornell University in 1930. In 1953, Dr. Asdell was a Fulbright professor in Denmark and lectured in several other European countries. He is author of Patterns of Mammalian Reproduction, *and part author of Dukes'* Physiology of Domestic Animals.

WHY are so few animal species domesticated by man for use as food for himself? We can name those of general use on the fingers of one hand: cattle, sheep, pigs, goats, fowl. Ducks, geese, and turkeys are somewhat restricted in their use, as are some species of fish and other birds, as well as rabbits and bees. I believe that a species, in order to be useful to man, must, first, be amenable to men's will and second, be adaptable—that is, must be able to adjust to the varied environment that man himself is able to endure. This means a large measure of variability.

At this point it would seem that the problems here are ones for the psychologist and the geneticist and not for the physiologist. I digress to ask what is the function of the physiologist? In the field of modern science, physiology seems to be disappearing as an entity. After some reflection I think that the best definition I can muster is that a physiologist is a person who seeks to define function and then to explain it. He has a split personality. As a research worker he has to analyze a problem and attack it with the tools of the anatomist, the biochemist, the statistician, the geneticist, and the embryologist. As an armchair philosopher, he has to synthesize, to bring together the results of his own work and those of others, and to produce a coherent whole. He is a veritable Humpty Dumpty of modern science, a philosophic Humpty Dumpty in Lewis Carroll's sense, not in the sense of the nursery rhyme.

The variability of the animals that serve man is a key point in determining their usefulness. In terms of the genetics of 10 or 20 years ago, the useful economic characters were defined as governed by multiple factors or modifiers. Nowadays the population geneticist has reinvestigated the question with new tools, those of path coefficients and such like. He has concluded that in many of the continuous variables, such as milk yield, inheritance plays only a small part in the total variability, whereas environment counts for a great deal. This puts me right back into a job as a physiologist. What is the optimum environment? How does the animal respond to it?

Further reflection shows that, in terms of modern physiological fashion, we have in the past tried mainly to explain differences among individuals of the same species in terms of their endocrine activities. Now we are steering away somewhat, and we are seeking explanations in variations in the physical environment, in climatic factors, in housing, and in nutrition and feed.

It is true that in some instances we can interfere with the endocrine glands, either by suppression or by reinforcement, and so produce quite a profound modification in economic worth. But my experience has been that each individual is very finely adjusted insofar as its endocrine system is concerned. If we upset the balance to our advantage in one direction, we pay for it in another. This does not

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matter when the animal has to be slaughtered almost immediately, but when we hope for a lifetime of production or reproduction from it, we are likely to be disappointed. This is especially true in the endocrine system dominated by the anterior pituitary gland, which, after all, regulates most of the factors of food interest to man. Perhaps the trouble is that we do not yet have assay methods for hormones in the live animal that are sufficiently accurate to make the necessary adjustments.

I am not sure that initial variability is by any means the whole explanation of selection for useful domestication, since it seems to me that a species tends to become more variable after it has been domesticated. The fox and the mink seem to be fairly constant in the wild, but under domestication many mutations modifying the color of the fur have arisen. Perhaps they also occur in the wild, but there they have low survival value. However, it seems to me that, throughout the evolutionary process, the mutation rate has risen whenever the environment has become richer and more varied. I am not sure that the gene is a definite substance. It *may* be the point of equilibrium in a reversible action. When the environment changes, the point of equilibrium is shifted and the result is a mutation.

Books that mention food during the Middle Ages give us the idea that the animal cuisine was more varied than it is today. I have never met anyone who has eaten swan or peacock peahen. Many more species were then kept in domestication or semi-domestication, such as deer and pheasant. But of course, in another sense, owing to the relative absence of vegetable and root crops, the main interest was on the animal kingdom for food. In recent times interest has been on fewer species of animals. This has come about through the concentration of the population into larger units, resulting in a need for more intensive and economical food production. There has been only one important addition to the list of animal foods in recent times, the turkey, and this species has been adopted by attaching itself to the menus of ritual feasts. Perhaps the new small turkey (at Cornell, 3½ lb) will be more acceptable for everyday use.

In the 1840's, the French naturalist Isidore Geoffroy St. Hilaire made a report to his government on wild species that might be added to the list of domesticated food animals. Soon afterward a society was formed to foster the introduction and trial of wild species into France. The larger kangaroos were reported as especially favorable, since they reproduce readily if they are given a fairly large living space and since their body proportions are such that they should give plenty of good meat. But no practical trials have yet been made. Darwin made the observation that carnivorous mammals adapt and reproduce better in domestication than do herbivores. But carnivores have to eat flesh, and the point of domesticated food animals is to convert *vegetable* protein into animal protein. Obviously, it is more economical for man to eat the flesh himself instead of via another carnivore.

However, I have the impression that we have been in too much of a hurry in the rather small amount of domestication work that has been done in modern times. Judging by the success some people have had with individuals of seemingly rather unpromising species, it would seem that many species could be tamed in time. Research over several generations might solve the difficulties of reproduction. We have paid only scant attention to this part of the problem. According to Darwin, vegetable-eating birds can be domesticated more readily than the flesh-eaters. There should be good possibilities among the herbivorous birds.

Besides showing efficient reproduction, a new food species must compete economically with the existing ones, and it must gain consumer acceptance. These are formidable hurdles, and in view of the continued gain in the world's population, and since it takes time to establish new eating habits, it is more likely that we shall have to concentrate on the few species we already have, even reducing the number somewhat, and improving their efficiency. The present tendency is to increase the number of breeds and strains within a species in an effort to find animals more suited to the particular kind of environment in which they are to be kept.

What are the main characteristics that make the species we now use seem indispensable? During World War II a good deal of attention was paid to the relative efficiency of our domestic animals as converters of energy and protein for human food. A summary of these investigations is given by Maynard in Table 1.

As a converter of energy the pig is the most efficient, but it uses large amounts of grains that could

Table 1. Efficiency, in percentages, of conversion of vegetable feeds into human food.

Species	Energy	Protein
Dairy cattle (milk)	15	14.5
Beef cattle (meat)	4	8.4
Hogs	20	14.5
Sheep	4	4.4
Poultry (eggs)	7	9.5
Poultry (meat)	5	13.4

be fed directly to man. The main points in favor of the pig are (i) its rapidity of growth, which gives the efficiency, and (ii) its extremely rapid reproduction rate. The pig population in this country closely follows the corn yield, and the pig population expands and contracts with this yield. It is this great flexibility in numbers that makes the pig so useful, even exceeding the Government as a stabilizer of crop fluctuations.

Sheep are relatively inefficient converters of energy, but they thrive on land that cannot be used for other agricultural enterprises, so they have a definite place in our economy. But the per-capita consumption of sheep and lambs is going down, and one feels that sheep are able to compete with more efficient species only because of their other product, wool. Sheep might almost disappear from the market if wool were no longer needed. Poultry is also relatively inefficient, but it has much the same position as hogs as a grain stabilizer. What poultry loses in efficiency, it gains in conveniences—the egg is so easily cooked in a variety of ways, and its preparation in the kitchen takes little time. To vegetarians like the Hindus, the egg is permitted as a source of animal protein.

Dairy cattle are comparatively efficient, and milk is an important article of diet, especially for the young. Even so, the dairy cow is beginning to feel the competition of vegetable fats. There is no substitute for beef as yet.

The work of the physiologist in increasing the efficiency of animal husbandry has been carried out in various ways, and it is possible to divide this work into four main periods representing different approaches, which are, to a considerable extent, epitomes of the historical development of the work.

The earliest approach was that of calorimetry and slaughter experiments designed to find out how much feed was needed for different functions, such as maintenance, growth, lactation, and fattening. This work was done mainly by the German calorimetrists, by Lawes and Gilbert in England, and by Atwater and Armsby in the United States. This was followed by the period of fitting feeds together so that they could be employed economically. The nutritive ratio expressed the relationship between protein and energy in a feed. More economical use of feed could be made if a suitable nutritive ratio could be set up for different functions. Much of this work stemmed from the need to make the best use of that wonderful, but nutritively unbalanced, grain, corn. This approach was, therefore, largely American.

The third period was that of analysis of growth impulses—of the mode of action of organs. Much of the work has been done in England, where Hammond and his students, together with McMeekan in New Zealand, Hirzel in South Africa, and Pälsson in Iceland, analyzed growth curves and applied the knowledge so obtained to livestock.

The nature of growth is governed by several growth gradients that differ in their effects upon parts of the animal as development proceeds. Thus, one series of gradients affects the growth of bone, muscle, and fat. These may be expressed as three overlapping curves, or semicircles, representing three successive phases. The first gradient is of bone growth, followed by muscle or protein growth, and finally by fattening.

During the period of most intense body or bone growth, it is difficult to fatten the animal. These gradients may be used to alter the proportions of these constituents of the animal. Low feeding early in life, during the bone phase, tends to produce short bones, whereas high early feeding tends to produce longer bones. High feeding in the fattening phase produces a fat carcass, and low feeding produces a lean carcass. Thus, the proportions of bone, lean, and fat can be modified at will. These methods have been applied with success in feeding individuals, but for economic use a method has to be applicable in mass feeding. This has not proved very successful. For one thing, there are always greedy and backward animals in each feed lot, and the rate of feeding is not easily regulated.

It seems, too, from this type of work, that the percentage of fat in the boneless cut is an important factor in palatability: 40 percent and the cut is perfect, 43 percent and the meat is a failure. However, at present, the trend is away from such refinements. Modern young people are content to eat their hamburgers and hot dogs from the kitchen stove, and they and most of their elders reach for a packaged cut in which it is nearly impossible to see what they are getting. However, quality is needed; leaner meat than we used to like is also needed, since it keeps better; and above all, uniformity is needed. This last is most important.

Another series of growth gradients affects the body proportions. There are three of these gradients. The first gradient travels from head to tail along the axial skeleton, making the newborn animal have a relatively large head and forequarters. The second gradient affects the appendicular skeleton and travels from feet to back. The long legs of the foal give a good idea of the effect of this gradient. The third gradient affects the axial skeleton again and travels from the tail, spending itself by the time it reaches the head. The first two impulses have done most of their work by the time

most domestic animals are born, but the third expresses itself mainly after birth; thus, it can be modified by suitable feeding. High feeding in the early growth phase, after birth, gives larger hindquarters, relatively smaller forequarters, and early maturity; and the more desirable cuts are developed. (The shape of the animal is materially altered.) Selection for the ability to respond to improved feeding has helped to produce this modification.

In recent years, the fourth approach has been added—"stretching the feed." It includes the administration of hormones, of sulfa drugs, and of antibiotics. These increase the efficiency of the utilization of feeds and get the most rapid growth possible out of the animal. This trend is now in full swing.

Several years ago Graham and Turner of Missouri pointed out that one of the main differences between a meat animal and a dairy animal was in the degree of thyroid activity. The temperamental dairy cow has a higher rate of thyroxin secretion than does the more plethoric beef animal, which fattens readily. It has been found possible to increase the milk yield of cows by giving them thyroxin in their feed. The hormone increases the rate of metabolism of each cell of the body; hence, we have to pay for this increased milk twice, once by the amount of feed required to produce the milk and once by the requirement of the generally faster metabolism. Present indications are that the faster pace is hard on the cow, and that it does not produce as much milk in later lactations. Conversely, the thyroid can be depressed to hasten fattening by feeding one of the antithyroid drugs, such as thiouracil. When this is done, less feed should be needed for maintenance and more feed should be available for fattening. Application to pigs has shown that the method is fairly effective during the later fattening stages, but if the thiouracil feeding is started too early, the pig is stunted and does not grow well. It grows well-rounded hams remarkable for its age, but they are miniature hams. That is, the pig is cretinized. Another difficulty is that the thiouracil-treated animal tends to have tissues that are a little more watery than is normal; edema is a symptom of myxedema. A judicious sequence of thyroxin to promote growth and of thiouracil to promote fattening has been recommended.

The injection or implantation of estrogens has a twofold effect. The first effect is that the resulting physiological castration tends to quiet the animal and allow it to fatten more readily. The second effect is to suppress bone growth and promote economical deposition of fat. This has become a method of fattening that finds favor with poultry growers, and it has possibilities in finishing other species for market. The other sex hormones, androgens, tend to promote the deposition of muscle protein, but they toughen the muscle and give it too strong a flavor.

In meat production a judicious mixture of estrogen and progesterone is now being tried, and preliminary results are favorable. Some of the bad effects of estrogen seem to be held in check by the progesterone.

Estrogens may be injected or implanted as tablets into cattle to promote mammary growth and lactation. The main difficulty is that they cause the female to become nymphomaniac and sterile. This too is waste in the long run, but estrogens can be used with profit on animals that are already sterile. Perhaps the difficulties could be overcome if we were able to give the proper balance of hormones instead of only one hormone. When we have better assay methods for the steroid and pituitary hormones, we shall be in a much better position to use them rationally. One of our difficulties is that the bovine animal works at a hormone level only one-twentieth or less than that of the human level. The cow is so much more sensitive to hormonal effects. This does not merely mean that we must use 20 times as much blood or tissue for assay; it also means that we must get rid of that much more impurities before we begin our assays, so that they are correspondingly more inaccurate. A beginning might be made in readjusting the hormone balance in this instance by ovariectomizing the cows after lactation has been established.

The possibilities in the use of growth hormone are obvious, but it is too expensive at present to be within the range of practical application. However, this must be emphasized. It does not follow that the most rapid rate of growth is the most economical in the long run. The advantages of rapid growth in animals that are to be killed for meat are obvious. Overheads, such as labor and cost of housing, are cut down with a rapid turnover, and the amount of feed needed for maintenance is reduced. But, is rapid growth conducive to lifetime productive efficiency? Evidence is accumulating that it is not. McCay showed years ago that rats that were grown slowly lived longer than did those that were allowed to grow at the usual rate. Now we have an experiment at Cornell in which calves are raised on different levels of feeding until they have their first calf, after which they are put on normal rations for the rest of their lives. The slowest growing group is yielding the most milk in the first lactation. A somewhat similar experiment in Denmark is giving

much the same results. When lifetime records on these animals are available, it will be possible to decide whether enough extra milk is produced to make the slower rate of growth worth while. We may have to fit the rate of growth or the kind of calfhood feeding to the subsequent function of the animals, if we are to get the greatest efficiency out of them.

Even for growth and fattening, I think that we are losing something in insisting upon rapid growth with purified and semisynthetic rations. I am sure that the barnyard fowl and pig that grew slowly and had to scratch or root for a living had a much better flavor than the modern products. And I believe that this applies to fruits and vegetables as well. The present-day giant varieties grown in climates and soils that give the maximum rate of growth also lack flavor. Flavor has probably passed out with the passing of the epicure. How far flavor counts with the average human being is difficult to evaluate, but maybe the heart does not grieve for that which it has never known!

The use of antibiotics in feeds may serve two purposes: it may increase the efficiency of cellular metabolism, and it may cut down the number of symbiotic bacteria that feed upon the material provided for the animal. In the use of antibiotics, new problems inevitably arise. Modification of the bacterial flora may reduce the amounts of certain substances available to the animal. Thus, the use of certain antibiotics in chick rations has been followed by hemorrhagic symptoms resulting from the failure of bacteria to synthesize sufficient vitamin K, the blood-clotting vitamin, for the needs of the host organism.

It is really rather surprising how little we actually know of the underlying physiological causes of differences in animals of the same species. In meat and dairy cattle the observed differences in thyroid activity are not the whole answer. Thyroid differences between meat and egg-laying poultry have been observed, but when we say this we have only scratched the surface. No one has been able to turn a meat animal into one of dairy merit, or vice versa, by thyroid treatment. Attempts to produce breeds that will combine superior merit in both categories have not met with much success, and it is open to question whether they ever will. The evidence seems to suggest that we are dealing with incompatibles.

A new trend is to be found in the work now in progress that is designed to find out how well and in what ways animals adapt themselves to various climatic factors. This is especially important, since the breeds of dairy cattle that have been evolved in temperate zones do not usually do well in the tropics, the very areas of the world in which there are immense numbers of people on a low standard of living, particularly with regard to their supply of animal protein. I am not sure that we yet know how to feed animals properly in the tropics. The nutritive ratio provided may be wrong, especially for maintenance. More protein and less energy may be needed in hot climates. Heat loss too is an important factor, and the physiologist, together with the engineer, can do much to provide an environment in which a maximum heat loss can be made to occur, so that the body temperature of an animal does not become too high for efficiency. The testing of heat losses from animals of different skin color and different hair color and texture is also a part of this work that is still in the experimental stages.

In summing up, we find that the presence of a large number of people on a finite area of cultivable land tends to reduce the diversity of the farm industry. As I pointed out earlier, the tendency is to concentrate on a few species of animals and to increase the efficiency with which they can convert feed into food. Looking ahead, perhaps we should insure for the future by inquiring into the efficiency of some of the more rapidly reproducing species that are not yet used to their fullest extent.

Many rodents reproduce rapidly, such as the rat and agouti. I do not know whether any efficiency figures are available for these animals comparable with those I have shown for the commoner farm animals. The rabbit is another species that offers great possibilities for further exploitation. This species thrives in a confined space and will live in a variety of climatic conditions. It is a rapid reproducer and the flesh is mild and easily cooked. Above all, the rabbit is a strict vegetarian. It can be fed on many plants, either as hay or as green feed. In this respect it is superior to the rat, which requires much of its food in a form that can be used directly by man. It is surprising that so little has been made of the rabbit as a major producer of the world's animal protein.

Looking still farther into the future, the animal farm may become obsolete and the commercial packing houses will be offering us meat that has been grown and cultured in suitable mediums, perhaps alfalfa and timothy-hay infusions judiciously reinforced with vitamins and hormones. This product will be wafer thin, drawn off on rollers, and presented to the public suitably and attractively packaged!

Role of Genetics in Adapting Animals to Meet Changing Requirements for Human Food*

T. C. BYERLY

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WE in the United States eat rather well. We are eating at the rate of about 150 lb of red meat, 75 lb of poultry meat and eggs, and about 350 lb of fluid milk annually per person. These values for red meats and poultry meat and eggs are about 5 times the amounts available to the average citizen of the world. We eat animal products because we like them and because they provide proteins of high quality and essential vitamins.

We have these foods we want because during the past few years we have diverted many acres from the production of horse feed to the production of feed for food animals and because we have been able, through research, to increase the output per breeding unit for all classes of food animals by about 20 percent during the past 20 years. Different classes of livestock show widely different rates of increase.

Although our supplies of animal foods have kept pace with population increase, they will continue to keep pace only through the development and application of more effective breeding methods. Our population, and that of the world, is increasing at about 1.5 percent per year; we can gain only limited acres by eliminating horses and extending the area and productivity of crop and grazing lands. Improvement in the efficiency of production of food animals through nutrition and other environmental means is limited by the genetic capacities of our livestock. We may elect or accept a diet containing increasing proportions of cereals, potatoes, synthetics, substitutes, and plankton. We may continue to have the animal foods we like—if our geneticists, animal breeders, and livestock producers develop and apply more effective breeding methods. The basic problem that challenges geneticists in all classes of livestock is the efficiency of conversion of feed to human food and other useful animal products.

Efficiency of feed utilization varies with rate of gain per animal unit or, for milk and eggs, production per individual. This follows from the fact that an animal of given weight, age, sex, and kind has a maintenance requirement that must be met prior to net gain or production, although gain or production may be accomplished temporarily through utilization of reserves. Among steers of the same age, weight, and condition there is a very high correlation between daily gain and feed efficiency. A hen of 4 lb body weight requires about 60 lb of feed for maintenance per year. For each dozen eggs produced it will require an additional pound and a half of feed. Thus such a hen requires about 75 lb of feed if it lays 10 doz eggs in a year, or 90 lb if it lays 20 doz eggs. In the first instance, each dozen eggs costs 7.5 lb of feed, in the second only 4.5 lb. During the past 20 years we have achieved an increase in egg production of 5 doz per layer, from a level of 10 doz to a present level of 15 doz. This increase has accomplished a change in feed cost from about 7.5 lb/doz of 20 years ago to about 5.5 lb today, in commercial egg-production flocks. Similar gains in efficiency have been accomplished in the production of commercial broilers, especially during the past 5 years. Some increase in the efficiency of milk production and pork production has also been accomplished. With respect to beef cattle and sheep, improvement in the efficiency of feed utilization has been small.

A Montana No. 1 sow and its litter at the U.S. Range Livestock Experiment Station, Miles City, Montana. This stock was derived from original crosses of Danish Landrace and American Hampshires. It combines fecundity and meat type.

A considerable portion of the gains accomplished and to be accomplished depends on selective breeding. Heritability, which measures additive genetic effects, is substantial both with respect to rate of gain in weight and rate of production of milk and eggs. Estimates of the heritability of rate of gain vary from about 25 percent for pigs to more than 50 percent for cattle. Estimates of the heritability of milk and egg production are of the order of 25 percent. Abundant evidence has been adduced that heterosis effects can contribute to rate and efficiency of gain, above the effects of additive genes, about 15 percent.

The methodology of the application of genetics in animal breeding in the United States is based on the studies of Sewall Wright (*1*), Jay Lush (*2*), and their students and colleagues. Closed-flock and family-selection breeding with judicious use of performance tests of individuals, siblings, and progeny groups have been demonstrated to be effective with respect to additive genes. The development and evaluation of the combining value of inbred lines and of unrelated noninbred lines have demonstrated that important gains from heterosis can be achieved through first cross-matings of lines of demonstrated superior combining value. We need a sure method of combining the gains from additive genes and from heterosis. Perhaps the reciprocal recurrent selection method devised by Comstock (*3*) will prove to be the method.

Other major problems common to all classes of food animals are (i) fecundity, for which heritability estimates are generally low but heterosis effects important; (ii) carcass composition, and egg and milk quality, for which heritability estimates are substantial; and (iii) environmental adaptation, including genetic resistance to disease. There are also many special problems of particular importance in certain classes of livestock. I shall review current research on some of these problems and some of the work that is needed for their solution.

Half our corn crop is fed to pigs. To this extent, pigs and people are competitors for food—not now but in any time of stress. The pig also consumes several million tons of milling, packinghouse, and other by-products not normally used as human food. In return, our pigs supply us with about 60 lb of pork per capita, about 12 lb of lard we use in baking and other food use, and a surplus of more than 500 million lb of lard a year. Herein lies our genetic problem. Each pig of average market weight produces about enough pork for two people, enough lard for three. Pigs of the world vary with breeding and nutrition. The English Large White and the Danish Landrace, fed in their homelands largely on barley and fishery and milk by-products, produce carcasses with a maximum of lean meat and a minimum of fat. Under our conditions of corn feeding, they generally produce leaner-than-average carcasses but often grow at a slower, less profitable rate than our native breeds. In contrast, the Mangolica, a native of the Danube plain, may develop a fat cover 4 or 5 in. thick.

Our pigs, to be profitable, must wean large litters of fast-gaining pigs, efficient converters of feed. The sow eats at least 1200 lbs of feed during pregnancy and lactation. A litter of six pigs thus costs about 200 lb of feed each at weaning; a litter of eight, only 150 lb. Our average number weaned is about 6.5 pigs, an increase of about a pig per litter in 20 years. Eight pigs is a reasonable objective. However, heritability of litter size within herds is relatively low, and research projects in which selection pressure for litter size has been applied through several generations have yielded no demonstration that additive genes can be readily segregated and recombined to increase litter size at weaning genetically. There is substantial evidence that heterosis achieved by crossing breeds

or by mating widely unrelated inbred lines will increase the number of pigs per litter. The use of widely unrelated hybrid combinations, analogous to hybrid corn, results in a gain of about a pig per litter in number weaned in first-cross litters and about a pig per litter in number farrowed by first-cross females.

Faster-gaining pigs tend to be fatter at a given age than slower-growing pigs, and fatter pigs generally yield more pounds of dressed carcass per hundred pounds live weight. However, these relationships, while usual, are not absolute. Workers in several midwestern state experimental stations cooperating in the Regional Swine Breeding Laboratory at Ames, Iowa, and research workers at Beltsville have demonstrated that it is possible to develop lines and combinations of pigs that are fast gaining, have high carcass yield and a high yield of the preferred meaty cuts—ham, loin, shoulder, and bacon. The development and application of methods for the genetic identification and market identification of meat-type pigs on our farms can eliminate our lard surplus and assure our pork supply. This problem is among our most important potential applications of genetics to food animals to meet changing requirements. The food fat use per capita is a very stable 43 to 45 lb. But it is less expensive to produce soybean oil, cottonseed oil, and other vegetable fats than to produce animal fats. The geneticist must help our farmers produce meat animals of high quality with the minimum of fat compatible with the palatability of meat products.

Our problems with beef cattle include climatic adaptation, rate of gain, milk production, carcass quality, and dwarfism. We in the United States get about 65 percent of our total beef and veal from beef cattle, the remainder as a by-product of the dairy industry. Europe has few cattle raised primarily for beef, while India, with a very large cattle population, eats little beef. Our beef industry is a two-stage industry. Breeding herds in all sections of the country, but especially in the South and West, subsist chiefly on forage. Steers from these herds go to the feed lot, where their weight is roughly doubled on grain and other feed concentrates. Grass, in natural range areas, still grows as it will. Elsewhere, it is raised as a crop, and the cost is real and recurrent.

It takes about 2 tons of total digestible nutrients (TDN) to maintain a cow for a year. If it weans a 400-lb calf, that calf has cost about 10 lb of TDN per pound live weight. Feed-lot gain from 400 lb to market finish, when total carcass fat content is 25 to 30 percent, requires about 5 lb of TDN per pound. Some calves gain slowly, about 1.5 lb daily and finish at 800 to 900 lb; others gain at a more rapid rate, more than 2 lb daily and finish at slightly heavier weights, about 1000 to 1100 lb. Some do not finish until they reach an age of more than 2 years and weights of about 1500 lb. Consequent to small finish weight with slow rate of gain is high unit cost in feed but preferred carcass weight and quality. Rapid gain and small or moderate finish weight give an economical production of good quality beef, while slow maturing, heavy beef can be produced economically only with very inexpensive feed, usually principally forage.

In genetic research with beef cattle at Beltsville and at the U.S. Range Livestock Experiment Station at Miles City, Montana, Knapp and Nordskog (*4*), in cooperation with the Montana Agricultural Experiment Station, have established the fact that rate of gain is highly heritable, most estimates being greater than 50 percent. Within wide limits, it is possible to increase the rate of gain by selective breeding without substantially increasing the weight at desired market finish or diminishing the quality of the finished carcass.

The Department of Agriculture has initiated

A herd of Herefords used in genetic studies at the U.S. Range Livestock Experiment Station, Miles City, Montana. Brood cows subsist on range forage the year round.

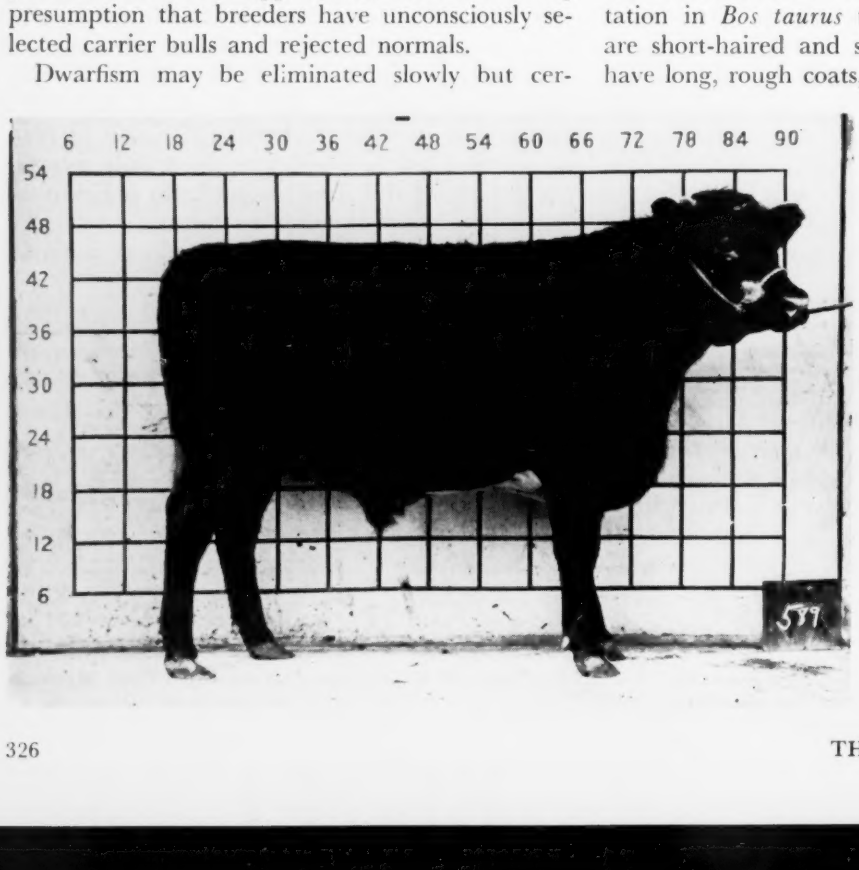
cooperative beef-cattle-breeding research with 38 states and the territory of Hawaii. The objectives of this program are the development of breeding methods suited to the improvement of beef cattle and the identification of lines of cattle with respect to their genetic capacity for rate of gain, market quality, reproductive capacity, and other factors of economic importance. More than 100 lines, comprising several thousand head of breeding stock, are available. Since rate of gain is highly heritable, performance tests based on measurements of individual and sib rates of gains of steers, bulls, and heifers under standard feed-lot conditions are used as a basis for selective breeding improvement in rate of gain. It is estimated that the output of beef per breeding unit can be increased at least 10 percent in the next 15 years through application of the results of this research.

The establishment and genetic evaluation of the many breeding lines in this cooperative project quickly revealed a widespread occurrence of genetic dwarfism. Phenotypic expression varies widely from calves obviously dwarf, and often hydrocephalous at birth, to calves that appear nearly normal but are slow in growth and end up at about half the normal adult weight. Extreme deviates may mature sexually and reproduce. Dwarfism is apparently inherited as a simple recessive. Since it is so widely scattered through our beef-cattle population and since known carriers are very often superior individuals in appearance, there is a strong presumption that breeders have unconsciously selected carrier bulls and rejected normals.

Dwarfism may be eliminated slowly but certainly by progeny testing and the elimination of proved heterozygous carriers. Paul Gregory (*5*) of the University of California has developed an ingenious device, which he has called a profilometer, that may be helpful in speeding up the elimination process. The profilometer measures the frontal skull contour. Since the homozygote is often hydrocephalous, with a frank frontal bulge, it is reasonable to postulate a slight bulge in the heterozygote. Gregory has studied profiles of progeny-tested horned Hereford bulls in several herds in which dwarfism is known to occur. He has been able to separate carrier and noncarrier bulls in these herds neatly by their profiles. It remains to be demonstrated whether the profilometer will yield accurate diagnoses in herds generally.

In the Gulf Coast area of the United States and in the tropical and subtropical areas of the world the Zebu, *Bos indicus,* comprises a major portion of the cattle population. Studies by Rhoad (*6*), confirmed by Brody and coworkers (*7*), indicate that the Zebu is more heat-tolerant than *Bos taurus,* as measured by the rise in body temperature as the environmental temperature rises. *Bos indicus* has a relatively large skin area with a characteristic hump, a heavy dewlap, and loose skin. *Bos indicus* is often light or tawny in color. On the other hand, it is slower to mature than *Bos taurus.*

Bonsma's (*8*) studies in South Africa indicate that both individual adaptation and genetic adaptation in *Bos taurus* take place. Adapted cattle are short-haired and shed normally; nonadapted have long, rough coats, which they may retain in-



A yearling Shorthorn steer on record-of-performance test at the Agricultural Research Center, Beltsville, Maryland. Rate of gain is highly heritable and may be increased by use of bulls whose steer half sibs gained rapidly under standard record-of-performance feeding conditions.

A group of heifers derived by selective breeding from Zebu-Angus crosses at the Iberia Livestock Experiment Farm, Jeanerette, Louisiana.

definitely. Adapted animals mature sexually at normal age and reproduce regularly; nonadapted animals mature slowly and reproduce irregularly. The principal beef breed in South Africa is the Afrikander. These are big, red beasts that thrive in the semiarid hot climate. First crosses between *B. taurus* and *B. indicus* result in calves heavier at weaning than those of the parental breeds and in first-cross females which wean calves heavier than those weaned by females of the parental stocks mated to similar bulls, according to a recent summary by E. J. Warwick (1952). It has been assumed that Zebu and Zebu crosses are more productive in the tropics than *B. taurus* breeds because of a greater heat tolerance. The difference in heat tolerance has been established. Evidence of a causal relationship between the heat tolerance of the Zebu and productivity remains to be demonstrated. Certainly heterosis is an equally probable cause of superior first-cross performance.

Superior first-cross performance has impelled many breeders and geneticists to develop new breeds by selection among the advanced generations from the original crosses. The King Ranch in Texas, during a 30-year period, has developed the Santa Gertrudis from original Shorthorn-Zebu crosses. Tom Lassater in Colorado has developed the Beefmaster from a crossbred base of Zebu-Hereford and Shorthorn. He is selecting only for weaning weight and quality and female reproductive performance. Mating among the selected individuals is random, color is various. It will be interesting to follow the future development of this herd with respect to performance and heterosis.

The USDA, in cooperation with the Louisiana Agricultural Experiment Station, has since 1926 selectively bred from original Zebu-Angus crosses. Segregation for color, size, conformation, and reproductive performance is continuous. Excellent individuals occur each year. It seems probable that a degree of stability can be reached through selection based on family performance, but whether we shall have to accept a random-breeding, heterozygous population or can, through accumulation of additive genes, establish superior stable lines remains to be determined.

These highly heterozygous populations in the South are a real challenge to the geneticist. Koger in Florida has initiated a modified recurrent selection program in cooperation with breeders, using progeny performance tests of purebred bulls in

A Zebu bull (*Bos indicus*) used in breeding studies at the Iberia Livestock Experiment Farm, Jeanerette, Louisiana. Crosses of *B. indicus* and *B. taurus* result in substantial heterosis. Crossbred calves exceed those of purebred parental stocks by about 10 percent in weight at weaning.

commercial herds of dissimilar breeding as a basis for breeding improvement of the parental lines. Projects of this sort emphasize the need for discriminating blood-group analyses to determine paternity in random-mated beef herds. Projects in Wisconsin and Wyoming are directed toward the determination of the blood-group situation in beef cattle.

The old problem of the genetic compatibility of superiority in several important traits is very important in beef production. At the one extreme, some lines of beef cattle, genetically superior in conformation and market finish, produce so little milk that nurse cows are used to rear their calves. At the other extreme, some of our dairy cows give extremely large yields of milk, but their male offspring are killed at birth, as not worth the feed and labor required to carry them to veal or heavier weights.

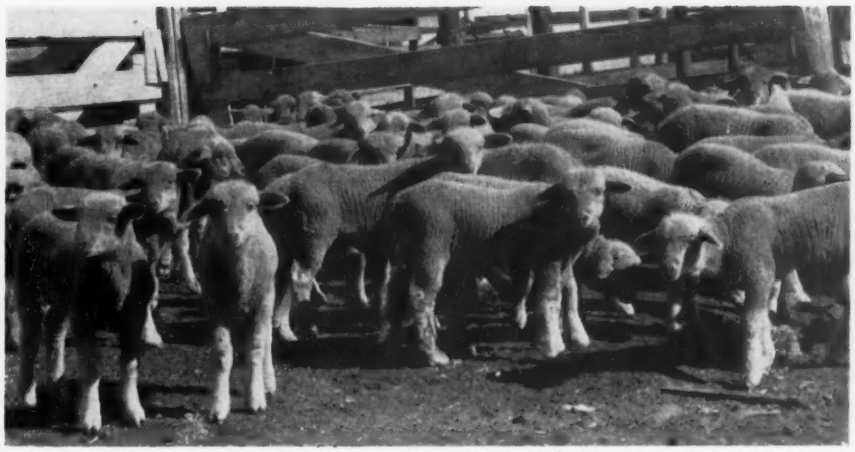
Certainly beef cows can be bred to produce enough milk to wean a heavy calf. Gifford (*9*) at Arkansas has measured milk production in relation to weaning weight in beef cattle. The relationship is substantial. On the other hand, our heavier dairy cattle, such as Holstein and Brown Swiss, and our dual-purpose breeds, such as Milking Shorthorns and Red Poll, produce fast-gaining calves. Their conformation and rate of finish are mediocre but can surely be improved. The heritability of milk production is only about half that of rate of gain. A slow, progeny-test improvement of dual-purpose cattle may be of increasing importance as human population increases.

The production of milk per cow in the United States has increased by more than 1000 lb, or by about 20 percent in the past 20 years. This has been accomplished largely through the use of superior sires, especially progeny-tested sires and their sons. Artificial insemination has helped greatly in spreading the superior germ plasm of superior sires. The number of cows bred by artificial insemination has increased from about 10,000 in 1939 to more than 3½ million in 1951.

Research in the USDA indicates a substantial increase in milk production because of heterosis in first crosses among *B. taurus* breeds of dairy cattle. Research with crosses of the *B. indicus* Sindhi dairy breed in crosses with *B. taurus* dairy breeds may lead to improved milk production in our Gulf Coast area, whether from heterosis or from heat tolerance of these *indicus-taurus* crosses.

Among sheep, genetic diversity is very wide. In the amount of wool produced, they vary from the hair sheep of Africa, with no wool, to exceptional individuals in several breeds that have produced 12-mo fleeces weighing more than 20 lb. In character of wool, they vary from the Tasmanian Merino, which may produce wool of less than 20-μ

A flock of Hampshire ewes at the Agricultural Research Center, Beltsville, Maryland. The Hampshire is noted for quality of lambs, both from purebred and crossbred matings.



A group of Targhee lambs at the Western Sheep Breeding Laboratory, Dubois, Idaho. This new range breed produces heavy fleeces of fine wool and large lambs of good quality.

diameter, to the rug wool sheep of Southwest Asia, with wool of 40-μ diameter or coarser. The Karakul, which produces the Persian lamb of commerce, is among the coarse wool breeds. It is noted, of course, for the attractive curl and luster of the pelts of unborn or newborn lambs. Many sheep in southern Europe are bred for milk production.

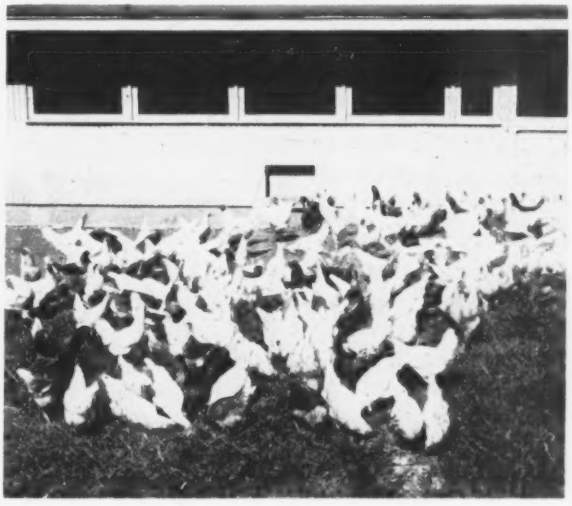
Genetic diversity in the world's flocks and herds would be very useful to geneticists and animal breeders working with sheep and cattle, and pigs too, if we had access to the stocks of the world. Unfortunately, the continued existence of foot-and-mouth disease, rinderpest, and other epizootic diseases among the livestock of most countries of the world makes their stock unavailable to us. One of our pressing needs is the development of safe methods and facilities for the importation of new genetic stocks without an accompanying disease hazard.

In the United States, as in the rest of the world, sheep have utilized areas where grazing is sparse, with special areas in more fertile regions where lambs are fattened for winter market or produced for the Easter market.

Our basic breeding flocks in the eleven Western States and Texas, which together have two-thirds of our total sheep population, are of Rambouillet breeding, which is in origin the French version of the Merino. Many of the flocks are mated to Suffolk or Hampshire males for the production of superior market lambs. The crosses grow faster, more are ready for market at weaning, and carcass quality is superior to the Rambouillet.

In cooperation with the eleven Western States and Texas, the USDA operates the Western Sheep Breeding Laboratory and Range Sheep Experiment Station with headquarters at Dubois, Idaho. Breeding flocks of Rambouillet and of two new dual-purpose breeds, the Columbia and the Targhee, are herded on spring-fall sagebrush range, spend the summer in the mountains, and the winter on hay in an irrigated area a few miles from headquarters.

The general objectives of research at this laboratory are the development of breeding methods and improvement in fleece weight and quality and of lamb weight and quality through selective breeding. The Rambouillet band is divided into about 30 inbred lines, a phenotypically selected control, and a genetically stabilized or random-selected control. Selection based on an index weighted for heritability and importance has been applied within lines. Substantial progress has been made in eliminating skin wrinkles and wool blindness. The latter character so restricts the foraging capacity of the wool-blind ewe that on the average (Terrill, *10*) she weans 10 lb less lamb than her open-faced sister.



A flock of "hybrid" pullets at the Agricultural Research Center, Beltsville, Maryland. Hybrid combinations of selected inbred lines reflect heterosis in high rate of egg production.

A flock of Beltsville Small White turkeys ready for the holiday market. A young Beltsville hen supplies abundant meat for a family meal.

Here, as in every class of livestock, our basic problem is to improve the quantity and the quality of the product per animal-breeding unit. We seek methods for improvement through an accumulation of additive genes with favorable effects and with maximum benefits from gene interaction.

The impact of genetics on poultry breeding is currently heavy. Several large commercial breeders are producing chicks by the development of inbred lines, followed by selection of the lines that give superior cross-line progeny with respect to egg production. The selected lines are propagated and combined each year for the production of females for commercial egg production. It is estimated that about 10 percent of the national laying flock consists of chickens of this type of hybrid breeding.

Many of our state agricultural experiment stations are studying methods of developing and testing inbred lines and hybrid combinations. Many of these combinations are tested cooperatively with the U.S. Department of Agriculture at a central testing station located near the Purdue University campus at Lafayette, Indiana. Experimental hybrid combinations are tested for egg production and other economic factors in the same environment at the same time in comparisons with selected commercial hybrids, crossbreds, and standard strains. The experimental hybrid combinations vary widely in performance. The best of them compare favorably with the better commercial hybrids.

One problem of special interest in this connection is that of genetic resistance to the avian leukosis complex. This disease is neoplastic and associated with an infectious agent. It is the most important cause of mortality among laying chickens. Effective prophylaxis, treatment and methods for early diagnosis are lacking. Hutt and Cole (*11*) at Cornell University and Waters (*12*) and his coworkers at the Regional Poultry Laboratory at East Lansing, Michigan, and others, have developed susceptible and resistant strains of chickens through selective breeding. However, it has been determined that crosses of resistant and susceptible lines are sometimes more susceptible than the susceptible parental line or, conversely, the neoplasm is more vigorous and thus more frequently fatal in the hybrid than in the chickens of either parental line. Apparently, there are some lines which are themselves resistant to the avian leukosis complex and which transmit the genetic capacity for resistance to their offspring in sufficient degree to repress the expression of the neoplasm in their hybrid progeny. We have, therefore, the double problem of finding lines that will combine to produce hybrids superior for both egg production and for resistance to the avian leukosis complex.

Generation interval is a retarding factor in the genetic improvement of all livestock. It is most painfully retarding with respect to the development of methods. Even with chickens, a minimum of 3 years is required to produce the three generations necessary as a minimum effective test of a method. Seeking guidance from pilot tests, D. C. Warren, in cooperation with the Poultry Department of Purdue and with support of a Rockefeller grant, has initiated a study comparing genetic increase in egg production in *Drosophila* with closed-flock family selection, development and testing of inbred-line combinations, and reciprocal recurrent selection. This project has gone through two selection cycles totaling more than 30 generations. It is not yet obvious which of these methods will ultimately prove superior.

The development of inbred lines has led geneticists to postulate that the phenotypically superior lines may retain, through automatic or conscious selection, far more heterozygosity than their coefficient of inbreeding would indicate. Briles (*13*) at the Texas Agricultural Experiment Station has determined the existence of heterozygosity of blood-group genes in several inbred lines of chickens. These studies support the postulate of a retention of heterozygosity in such lines.

As a final example of livestock breeding research, let us consider the development of the Beltsville Small White turkey. The Beltsville Small White was bred to fit the needs of families for a bird of 5- to 12-lb oven-ready weight, as contrasted to the Broad Breasted Bronze which is characterized by 10- to 25-lb oven-ready weights, suited to the needs of large family gatherings and institutional users.

Crosses among several widely unrelated varieties of turkeys, including the wild turkey, White, Black, and Bronze varieties of standard weight, were made at Beltsville about 1935 under the direction of M. A. Jull, who was then in charge of poultry research. From the progeny of these crosses, genotypic and phenotypic selections based on pedigree, individual performance, and family performance including some sib and progeny tests were made by S. J. Marsden and C. W. Knox for weight at 24 wk of age, fleshing, fatness, and color. Within 7 years, color and average weight objectives had been achieved. For the next 10 years selective breeding was applied to achieve uniformity, to improve fleshing, and to increase egg production. Egg production has been extended from spring season to 12-mo production. Genetically, egg production has been increased by selection for nonbroodiness and early maturity. Winter egg production is supported by artificial lighting to lengthen our short winter days.

A new development in the turkey industry has been facilitated by lengthening the laying year and by the small size of the Beltsville Small White. Very substantial numbers are produced at all seasons, marketed at 12 to 16 wk of age and live weight of 4 to 10 lb for use as fryers or small roasters. From a foundation breeding flock that has generally included fewer than 200 individuals, there has developed a population that now produces about 15 million turkeys annually. Stock has been distributed to breeders in many countries.

To conclude, if we are to continue to eat the meat, milk, and eggs we want, our geneticists must, through research, give to livestock producers the methods for genetic improvement in the conversion of feed into these foods of choice. Additive genes, heterosis, and gene-environment interaction must all be combined in these more efficient methods. The problem is difficult but urgent if we wish to satisfy our appetites.

References and Notes

* Presented in the symposium "Species that feed mankind," AAAS meeting, Boston, Mass., 27 Dec. 1953.

1. S. Wright, *Proc. Am. Soc. Animal Production* (1939), p. 18.
2. J. L. Lush, *Animal Breeding Plans* (Collegiate Press, Iowa State College, Ames, ed. 3, 1945).
3. R. E. Comstock, H. F. Robinson, and T. H. Harvey, *Agron. J.* **41**, 360 (1949).
4. B. Knapp, Jr., and A. W. Nordskog, *J. Animal Sci.* **5**, 62 (1946).
5. P. W. Gregory, *J. Animal Sci.* **11**, 700 (1952).
6. A. O. Rhoad, *Proc. Am. Soc. Animal Production* (1938), pp. 284–295.
7. D. M. Worstell and S. Brody, *Missouri Agr. Sta. Bull.* 515 (1953).
8. J. C. Bonsma, *J. Agr. Sci.* **39**, pt. 2, 204 (1949).
9. W. Gifford, *J. Animal Sci.* **8**, 605 (1949), abstract.
10. C. E. Terrill, *J. Animal Sci.* **8**, 353 (1949).
11. F. B. Hutt and R. K. Cole, *Science* **106**, 379 (1947).
12. N. F. Waters, *Poultry Sci.* **24**, 259 (1945).
13. W. E. Briles, L. W. Johnson, and M. J. Barber, *Poultry Sci.* **32**, 890 (1953), abstract.

BOOK REVIEWS

The Psychology of the Criminal Act and Punishment. Gregory Zilboorg. Harcourt, Brace, New York, 1954. xi + 141 pp. $3.50.

THIS collection of seven papers by an outstanding psychiatrist, the 1953 winner of the American Psychiatric Association's Isaac Ray award, presents to lawyers, judges, penologists, and sociologists, as well as to psychiatrists and psychologists, one of the most cogent and arresting challenges faced by today's society: the need for decent, humane, efficient, and moral administration of criminal justice. The author thinks that we are not doing very well. His voice is not a solitary one. Justice Robert H. Jackson, speaking at the American Bar Center cornerstone ceremony in Chicago, said:

> The adminstration of our criminal law, from one cause or another, is a humiliation and a discredit to our profession and our country.

That Zilboorg's literary style is rich and flourishing, yet clear-cut and explicit, makes for pleasant as well as significant reading, but the grave significance of his challenge almost makes the reader feel guilty for enjoying his style so much. And perhaps the most significant thing about his challenge is this: here is a scientist—a doctor and psychiatrist—whose sympathetic interest in and open-mindedness toward other disciplines, notably philosophy and moral theology, give the best hope that the criminal and his problems will be appraised from the vantage point of a knowledge of the whole man—body, psyche, and soul. For it seems not too much to hope that psychiatry's willingness to give moral theology a hearing will be as helpful to the progress of psychiatry as has theology's acceptance of the findings of natural science been to the development of theological thought. Zilboorg's catholicity of source material is in refreshing contrast to the scientism that rejects all disciplines not based exclusively on laboratory techniques, and it results in an equally refreshing lack of scientific dogmatism.

But it must be conceded that the message of this volume is still chiefly negative. The one explicit and positive suggestion is that psychiatrists, on high moral grounds, should refuse henceforth to act as adversary witnesses in criminal cases, and should function only as friends of the court. The rest of the volume, despite the author's judicious self-restraint and spirit of reasonableness, is in substance a vigorous condemnation not only of things in the periphery of criminal law administration such as the orthodox test of legal responsibility—did the defendant know the nature and quality of his act or that it was wrong? It also is a condemnation of the very essence of the Anglo-American judicial method, the adversary proceeding. And his condemnation clearly brings into focus one dilemma which to date has prevented complete *rapprochement* between psychiatry and the law. For to Zilboorg,

> . . . whenever a system becomes formalized, procedure itself and technicalities of form and decorum acquire a greater importance than they actually deserve.

But the lawyer knows, or thinks he knows, that the history of liberty is largely the history of procedure. It is more than a coincidence that liberty has thrived best in the Anglo-American world where procedure has been litigious and adversary rather than officious and inquisitorial.

The "newer psychology," the author tells us

> . . . looks into the psychological depth of man and is inclined to disregard the worship of averages that never lead to the individual, still less permit us to gain any insight into his depth. . . .

He tells us that proper recognition of the dignity of man requires an entirely individualized appraisal of each person; each man is a law unto himself. But is this requisite total individualization adequately achieved in administration of criminal law by dichotomizing the trial of the accused from the treatment of the convicted? Apparently not, for Zilboorg's main attack is on the adversary trial. What he does not tell us is how to achieve total individualization in criminal administration and still preserve a rule of law.

> Psychiatry is therefore predestined to reject categories and preconceived notions and legal tests.

But liberty cannot reject legal tests. For men, even well men, cannot be trusted wholly at large. There is not only the problem of sickness, but there is also the problem of evil. Zilboorg himself admits that ". . . not every criminal is ill, not every crime is a result of psychological pathology." If legal categories are abhorrent to the psychiatrist, some of the things done in the name of psychiatry are abhorrent to the lover of liberty. For example, in psychiatry's name a law-abiding, capable, hard-working farmer, of good moral character, at least so far as his relations with the community were concerned, was condemned to indefinite commitment under the psychopathic personality law because of his ardent sexual nature. [Psychopathic Personality of Dittrich, 215 Minn. 234, 9 N.W. 2d 510 (1943)].

In sum, the author has taken a penetrating and thoughtful look into the depth of the chasm still separating law and psychiatry; he has not, at least in this volume, begun to build the bridge over the chasm. But in his willingness to let natural science team up with other disciplines, such as moral theology, perhaps at least he has begun to mold the bricks from which the bridge eventually may be built.

In one additional respect, however, Zilboorg seems constructive if only implicitly so. He thinks it questionable whether the criminal law's purported purpose of deterrence is substantially achieved. He argues that much of the motivation for stern enforcement of the

criminal law, however rationalized, is at root emotional, aggressive, and passionate.

> The conclusion therefore imposes itself upon us that the psychological, affective motivations in favor of punishment are the very same as the ones which lead men to commit a transgression of the law.

If this is so, and if psychology can delineate for us the rational from the instinctual motivations for punishment, I predict that lawyers will be as willing as the rest of men to concentrate on therapeutic treatment of the convicted, leaving to God, who alone rightfully claims it, the vengeance.

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Causality in Natural Science. Victor F. Lenzen. Thomas, Springfield, Ill., 1954. vii + 1212 pp. $3.

THIS little volume exemplifies a scientist's approach to a philosophical question. To ask "What is *x*?" from a scientist's point of view is to initiate a search for instances where *x* becomes manifest and to try to find what those instances have in common. When *x* stands for "causality," the manifestations are seen to be certain attitudes that men have toward events. Accordingly the first task is to list and classify those attitudes. Lenzen does this by describing the various frameworks of thought into which men's conceptions of the world have been cast: animism, organismic metaphysics, mechanistic metaphysics, positivism, and modern operationalism. He illustrates his descriptions by pertinent expositions of the corresponding highlights of philosophy, for example, Aristotle's classification of causes, the inductive schemes of Francis Bacon and J. S. Mill, Kant's *apriorism,* Hume's skeptical criticism of causal necessity, the "anti-metaphysical" formalism of Appell and Mach, Poincaré's conventionalism, and the operational analysis of concepts, which has become the philosophical backbone of modern natural science.

It appears to the reader of Lenzen's book (and justly so) that to talk about causality in natural science is to talk about the philosophy of natural science. This is true because much of natural science has to do with questions which, translated into the vernacular, begin with "Why . . .," and because the philosophy of natural science is concerned with deciding what sorts of answers are admissible.

The philosophy of science is a very large subject, and its treatment within 100 pages must necessarily be sketchy. Still the meat of the subject is there, at least inasmuch as it concerns the now closed field of classical mechanics and the very much open fields of relativity and quanta. There is also a chapter on biology in which the author discusses the question of reducibility of biological phenomena to physicochemical ones and introduces the reader to some methods of mathematical biology. The shift from deterministic to probabilistic conceptions of causality is touched upon in connection with Mendelian genetics, statistical mechanics, and the indeterminacy of subatomic events. In my opinion, the philosophical importance of this reorientation warrants a more extensive discussion, even within the modest scope that the author set for himself.

Lenzen's command of his subject matter is beyond question. It is somewhat difficult, however, to determine in what context the subject matter is treated, especially since a preface is lacking. The book is identified on the flyleaf as a Monograph in American Lectures in Philosophy. As a review monograph or an encyclopedic entry, the book certainly fulfills its function. However, if it is addressed to an audience, one may wonder to what audience it is addressed. Beyond the first four chapters, it says little to either the "intelligent layman" or the philosopher who is not rather thoroughly familiar with advanced physics. The author refers to the most profound notions of mathematical physics simply by name. Statements like "The principle of causality for relativistic field theory is a consequence of the mathematical theory of hyperbolic differential equations" (p. 80) or "radiations within an enclosure may be denoted by the phase assembly of photons which obey the Bose-Einstein statistics" (p. 86) can be properly addressed only to a physicist. An equation like

$$pq - qp = \frac{h}{2\pi i} 1$$

(p. 93) looks senseless to anyone who tries to interpret it in terms of conventional algebra.

If the book is admittedly written for physical scientists, then the question arises how much it can tell them that they do not know. The question is put here sincerely and not at all rhetorically. Since not many physical scientists are articulate about the philosophical foundations of their disciplines, it is anybody's guess how many of them are aware of the framework of thought that underlies their lifework and of how this framework came into being. Certainly this awareness is highly desirable. If there are many physical scientists who lack it, and if it can be inculcated by the extremely condensed exposition followed by the author, then the book is entirely justified.

ANATOL RAPOPORT
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Nobel Prize Winners in Chemistry: 1901–1950. Eduard Farber. Schuman, New York, 1953. x + 219 pp. Plates. $5.

SINCE the creation of the Nobel Foundation in 1901 to the end of 1950, 53 chemists have received Nobel prizes. The achievements of these men represent a series of major discoveries in chemistry in the 20th century. The author has written three sections about each prize winner. The first is a condensed biographical sketch which includes not merely a chronology of education and professional positions, but also the origin of the interest and conditions for the achievement of each individual. Then follows a selection from the prize-

winner's description of his work, taken most frequently from the lecture delivered upon his acceptance of the prize. A third section entitled "Consequences in theory and practice" indicates the influence of the discovery, whether of very broad scope and providing a new base for chemical thinking, or more narrow and leading to the solution of a definite practical problem.

The book is instructive, stimulating, and interesting. It provides the reader with a collection of short stories about great events in chemistry during the past 50 years, and a description of the brilliant personalities from which the discoveries originated. No chemical library should be without it.

ROGER ADAMS
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Know Your Reader. The scientific approach to readability. George R. Klare and Byron Buck. Hermitage House, New York, 1954. 192 pp. $2.95.

ACCORDING to the authors, "The purpose of this book is to provide writers with information that will help them to communicate." The information provided is essentially that sentences should usually be short, long words are to be avoided, and the reader should be personally involved whenever possible. All this may be determined by "scientific readability" tests, one of which provides us with the information that certain movie and confession magazines "could be read by 80 percent of the adult population." It is admitted, of course, that these readability formulas cannot do everything: "While the *New Yorker* rates highly readable by readability formulas, its appeal to readers is in great part the result of style aspects that readability formulas don't get at."

As one of the 20 percent who find the movie and confession things unreadable, and as one who is bored by the *New Yorker's* grade 7.7 chi-chi approach to cosmic problems (see the 7 April 1954 issue of *Punch*, please!), I rise to protest that if this is the sort of thing that readability formulas inspire, we would be better off without them. When we remember that this is supposed to be a book for professional writers, we realize that the style is both atrocious and in bad taste. It is full of chummy contractions, irritating inversions, and unnecessary words. The authors (who recommend Fowler's *English Usage*), evidently consider their effort to be an example of "readability." Sentences like this are all too common: "Related to a reader's background and interest is his purpose in reading." "Now there's still another way, but it's easy to run afoul on." "Much more important, though, we feel, is to say that every writer should care."

After such gems, it is consoling to find this concession: "Even future formulas, which may well consider some of the present immeasurables, will never take the art out of writing." Amen.

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A Field Guide to the Birds of Britain and Europe. Roger Tory Peterson, Guy Mountfort, and P. A. D. Hollom. Houghton Mifflin, Boston, 1954. xxxiv + 318 pp. Illus. + plates. $5.

ROGER TORY PETERSON is widely known for his bird paintings and for his guides (similar to this) to North American birds, and all the 1107 illustrations (publisher's count; Julian Huxley, in the introduction, says "more than 1200") and the explanatory page accompanying each plate are his work, while Mountfort and Hollom, each prominent and authoritative as a British bird student, provided the descriptive text, and the maps and distribution notes, respectively. All three have studied birds on extensive travels in Europe. Mountfort lived on the Continent for 10 years. Ornithologists from most European countries and the United States gave "particular assistance."

The book follows the well-known Peterson plan. Each plate shows a number of birds, brought together with their similarities in mind, drawn to the same scale and in the same attitude, with markers indicating their chief identifying characteristics, which are explained further but briefly on the page facing the plate. These excellent plates show nearly all species "in full colour," but many species appear in black and white too, in other attitudes, such as flying. End-paper drawings are of "Roadside silhouettes" and "Flight silhouettes." Two improvements over the American *Field Guides* are the frequent, additional, miniature pictures, in silhouette to show attitudes, in color to show color patterns as seen in flight, and the maps that show both the wintering and breeding ranges, in Europe, of 367 species.

The descriptive text gives the name of each species in several languages, including the North American, when one occurs on both sides of the Atlantic but has different names, as Sand Martin and Bank Swallow. (We call the Arctic Skua the Parasitic Jaeger, not "Arctic Jaegar," and the misprint in this error is very nearly the only one I find in the book.) The identifying characteristics (including flight, attitudes, and so forth, even nests, when helpful) are enlarged on, careful comparisons with similar species are made, and there are brief descriptions of calls, songs, and habitats. (Fuller accounts elsewhere do not say the Alpine swift uses mud in its nest.)

All the 452 species of Europe to about 30°E are treated thus fully, including vagrants such as the American robin. There are also briefer descriptions, without pictures, of 100 species of accidentals, "which have been recorded fewer than 20 times." (Better call *Apus affinis* the house swift, as in Asia, rather than "White-rumped swift," the official name in Africa of *Apus caffer*, in America of *Aëronautes saxatilis*. Our olive-backed thrush is brownish olive, not "grey-brown" unless badly worn.)

A fully illustrated chapter on "How to identify birds" is for beginners. There is a list of the 452 species on which you may check those you see on your next trip, and a list of the principal British ornithological societies, with addresses. It would have been well to mention that at least in England there are many local

bird clubs and natural history museums; for names and locations, consult the British Museum (Natural History), London S.W. 7. A boon to all users (other publishers please note) is the cross-referencing of plate to text and text to plate. Wisely, subspecies are not mentioned unless distinguishable afield. American users notice differences in classification—for example, the shearwaters are put in *Procellaria, Pterodroma* petrels in *Bulweria,* the great skua in *Stercorarius,* and the true sparrows and snowfinches are a family, Passeridae. It is time the British came around to using names such as gray heron, greater scaup, red kite, and red-billed chough instead of just heron, chough, and so forth, for definite species.

The book is of convenient pocket size, but the paper is poor for a book likely to be so constantly handled. Any English-reading bird student (visiting a country covered or a stay-at-home) interested in European birds will welcome this excellent book.

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The Climates of the Continents. W. G. Kendrew. Oxford Univ. Press, New York, ed. 4, 1953. 607 pp. Illus. $10.

FOR nearly a third of a century, *The Climates of the Continents* has been before the public, and it is still preeminent in its field. The fourth edition retains the essential plan of former editions, and it is apparent that Kendrew expects the formula that has worked so well in the past to continue in his favor. This formula, in essence, is a description of the climates, not so much of continents as of the countries within them, largely in terms of monthly and annual means of temperature and precipitation.

Frequent reference is also made to wind and pressure systems, agents that have traditionally served the climatologist as a device for generalizing upon local details of temperature and precipitation. In this edition the material in some areas has been enriched by the results of synoptic analyses (that is, the manner in which the monsoon loses its effect in passing westward over India, the cause of the winter rains along the tropical east coast of South America), a type of information essential to any explanatory treatment of climatic distributions. Other new material includes fuller data on sunshine and obstructions to visibility, and snow and ice conditions. In addition there are three chapters on North America that have not appeared previously: "Sunshine, cloud and fog" (35), "The climatic regions of Canada" (36), and "The climatic regions of California" (37). All continents have been given substantially more space, except Australia and Antarctica, and the number of climatic stations listed in tables has been increased to well over 500.

Any study based upon the political map is likely to give more intensive treatment to small countries than to large, and it is not surprising to find that the small but heavily instrumented countries of Europe are represented in the aggregate by more pages than are given to either North or South America. Asia and Africa form larger sections than the European, however, and the American reader will find much of interest abroad. And, whatever one may think about the wisdom of organizing regional climatology upon a country basis, it must be admitted that this arrangement makes it easy to refer to the text and tables.

For a volume so thoroughly revised, it is to be regretted that space could not have been found for maps of terrain, for they would be of the greatest aid in following remarks about the climates of regions with complex relief. Also, minor errors have been carried over from the older editions (Cherrapunji is cited as the rainiest point on the globe, but a well-known record of 20 years for Waialeale shows a higher annual mean; the lower Colorado basin is given as the warmest region of North America, outside Mexico, in the winter season, even though southern Florida must be accounted warmer still), and a few appear in the new chapters on North America (the eastern slopes of the Sierra Nevada of California are not more gentle than their western slopes).

Both the author and publisher are to be commended, nevertheless, for compounding such an extensive volume on regional climatology in a day when professional meteorological journals are monopolized by quantitative analyses very different in spirit and intent. Perhaps it is this very trend that gives new importance to a descriptive, regional climatology.

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Personality through Perception. An experimental and clinical study. H. A. Witkin, H. B. Lewis, M. Hertzman, K. Machover, P. Bretnall Meissner, and S. Wapner. Gardner Murphy, Ed. Harper, New York, 1954. xxvi + 571 pp. Illus. $7.50.

THE audience for this book will be select but interested. The prospective reader should note the subtitle, *An Experimental and Clinical Study,* for this volume presents the results of a 10-year program of basic experimental research on the relationship between a specific type of space perception—perception of upright position—and personality variables. This report should receive the careful attention of experimental perception psychologists, clinical psychologists, psychiatrists, and others interested in personality theory.

The major objectives were to determine the stability of an individual's characteristic methods of space perception, the generality of these methods to nonspace perception, and the relationship between these characteristics and the individual's personality organization.

Perception results were derived from the use of three standardized space perception tests: rod-and-frame, tilting-room-tilting-chair, and rotating-room tests. Nonspace perception and "body action" tests were also used. Personality factors were evaluated by use of an inter-

view, the Rorschach cards, the Figure-drawing test, the Thematic Apperception test, and case histories. The bulk of the data were obtained from 52 men and 51 women volunteers from college classes. Children from 8 to 17 years of age and a group of 77 mental hospital patients were also used.

The main impact of the over-all research program is the finding that individuals do show a stable difference in the way they respond to the objective characteristics of the figure-ground space tests. These results are relatively independent of dynamic factors and cannot easily be accounted for in terms of learning variables. In summary, the authors state,

> . . . there are individual differences in ease of separating an item from its context, related to the person's tendency toward active coping or passive submission with respect to the environment (including people) and the nature of his self-concept (p. 489).

It was noted that women are less analytic than men in the sense of being more affected by the contexts in which the figure (item) occurs. The hospital group confirmed the findings from the normal subjects, as did the results from the children.

The report represents a significant contribution in its own right as well as a *rapprochement* between traditional laboratory procedures and methods of describing human personality. It is true that the generalizations must be confirmed in other perceptual situations and with personality analyses differing from those herein reported, but, at least, the challenge has been given. Future research in perception and in personality organization will show frequent reference to this book.

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The Natural History of Mammals. François Bourlière. Knopf, New York, 1954. xxi + 363 pp. Illus. + plates. $5.

THIS translation of the author's *Vie et Moeurs des Mammifères,* published in 1951, is really a revision of that book. It is offered as a worldwide introduction to field mammalogy. If the title leads anyone to expect a rambling or anecdotal treatment of assorted facts, or a fact-rich but idea-poor compendium, he should examine this book closely and enjoy his surprise.

The subjects of the 10 chapters are as follows: locomotion; food and feeding habits; home, territory, and home range; defense and protection; sexual life and reproduction; development and longevity; migrations; social life; mammals and their environment; and, finally, the structure and dynamics of natural populations. There follow a taxonomic list of species and bibliography, the latter 26 pages in length, helpfully subdivided and in part annotated. The figures are pen drawings, all very clear and effective. The plates consist of well-chosen halftones.

The general excellence of this book is such that the few minor slips one finds are hardly worth mentioning. The indexing could have been better, and a few easily discerned errors could have been eliminated, as for example the two-way disagreement between the legend of the figure showing a murine oppossum opposite page 232 and the appended taxonomic list. Perhaps the topic of locomotion as an opener gives the book a slow start, but, as a whole, the text leaves little to be desired from the standpoint of both general reader and specialist. The book is also well suited for collateral reading in courses in vertebrate ecology and mammalogy. Recent work is stressed throughout, and American students will find helpful leads to important studies in the European literature.

The author is associate profesor of medical biology at the University of Paris. Trained in medical fields and an authority on geriatrics, he has maintained an interest in field zoology and has given us a book on natural history that upholds well the European tradition of vigorous interest in that subject. It is not too surprising, then, that Bourlière's book is a distinctly superior product in its field, and I recommend it highly because it is authoritative and exceptionally well written, and in particular, because its factual content has been organized effectively, in accordance with modern approaches and thinking in ecology.

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Proceedings of a Conference on the Utilization of Scientific and Professional Manpower. National Manpower Council. Columbia Univ. Press, New York, 1954. xii + 197 pp. $3.50.

Policy for Scientific and Professional Manpower. National Manpower Council. Columbia Univ. Press, New York, 1953. xix + 263 pp. Illus. $4.50.

THE first book contains the proceedings of a 5-day conference of 66 national leaders in the engineering, medical, educational, scientific, industrial, governmental, and military fields, conducted by the National Manpower Council at Arden House, Harriman Campus, Columbia University, 7–11 Oct. 1953.

The National Manpower Council, appointed by President Eisenhower while he was president of Columbia University, and financed by the Ford Foundation, is one of a large number of organizations, both within and without the federal government, that have been concerning themselves during the past few years with the serious national problems arising from the shortages of scientific and professional manpower.

Previous books published by the Council are *Student Deferment and National Manpower Policy* (Apr. 1952) and *A Policy for Scientific and Professional Manpower* (May 1953). The latter book sets forth five broad and related objectives for a cooperative effort involving government, industry, educational institutions, and professional and other groups which the Council believes can provide the nation with the scientific and profes-

sional manpower it requires. These objectives are (i) to develop more reliable knowledge about our human resources, (ii) to strengthen the institutions that educate and train our scientists and professionals, (iii) to maintain a continuous and large flow of students through our colleges and universities, (iv) to expand the opportunities for capable young persons to secure a higher education, and (v) to improve the utilization of the available supply of scientific and professional personnel.

The Oct. 1953 conference concerned itself with the last of these objectives. It centered its attention on the three professional fields of engineering, medicine, and teaching, which the Council states are marked by distinctive if not unique utilization problems. The effective use of the personnel in each of these fields is of critical importance to the nation's well-being.

The proceedings of the October conference were prepared by Henry David, executive secretary of the Council. Part 1 contains the challenging papers presented to the entire conference together with discussions of them. Part 2 includes the papers presented to the three working groups—engineering, medical, and teaching—and an abstract of the discussions in each group as well as of the discussions in the general conference when the group findings were presented by the respective chairmen in the form of a list of points on which the conferees had agreed. The engineering group presented 12 points, the medical group 8, and the teaching group 7.

Instead of summarizing the many recommendations made by the three discussion groups, Eli Ginzberg, Director of Research of the National Manpower Council, attempts, in Part 3, to distinguish six major approaches for securing more effective utilization of scientific and professional manpower. He expresses the belief that these approaches would facilitate the organization of the many recommendations and would disclose the extent to which specific techniques for improving utilization in any one of the three fields—engineering, medicine, and teaching—are applicable to the others. Dr. Ginzberg tests the validity of his scheme by citing, under each approach, concrete examples drawn from the three fields represented at the conference. Except for the conference program and a list of participants, the discussion by the general conference following Dr. Ginzberg's remarks concludes the book.

Dr. Ginzberg's six major approaches are (i) the transference of work from professional persons to technicians and assistants; (ii) the increased use of capital to permit the full employment of professional skills; (iii) incentives to prevent the wasteful turnover of professionally trained personnel; (iv) new work patterns to improve the total output of professional and semiprofessional personnel working in concert; (v) reassessment of the appropriateness of the education and training of professional persons; and (vi) improved leadership and administration.

The findings of the three discussion groups and of the entire conference, and the discussions that followed their presentation, underline the fact that the current and prospective shortages of scientific and professional personnel have been powerful stimuli, not only for an increase in the supply, but, perhaps even more significantly, for improved utilization of scientific and professional manpower. This should be of value to persons in authority in any professional field, and the six "major approaches" can well form the basis for a sound approach to manpower problems in any professional field.

HENRY H. ARMSBY

Office of Education,
U.S. Department of Health, Education, and Welfare

Number: The Language of Science. Tobias Dantzig. Macmillan, ed. 4, New York, 1954. ix + 340 pp. $5.

THIS is the fourth edition of a book that is now well known, having been before the public for about a quarter of a century. As the author knows, part of the book is merely a reprint of what has already been published. Part 2, "Problems old and new," deals with recording of numbers, topics in integers, roots and radicals, and principles and arguments. On the whole, the new part is well integrated with the old. As one criticism, the statement that a certain topic is beyond the scope of this book occurs too frequently. It is difficult to see why something that is not elaborated is worth mentioning at all.

E. T. BELL

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California Institute of Technology

Basic Bacteriology. Its biological and chemical background. Carl Lamanna and M. Frank Mallette. Williams & Wilkins, Baltimore, 1953. 667 pp. Illus. $10.

SINCE its introduction, Lamanna and Mallette's *Basic Bacteriology* has had an opportunity to be appraised by two highly critical and divergent groups—teachers and students. Within my experience and my class of graduate students, it has met the test wonderfully.

If the new reader equates the titular "Basic" with "elementary," he will soon be disappointed. This presentation of the fundamental physical, chemical, and biological problems in bacteriology purposefully integrates cognate sciences with bacteriology in erudite fashion. It treats bacteriology as a distinct basic discipline of science with little reference to the applied aspects usually stressed; it introduces subject matter new to most textbooks; it treats traditional aspects with skeptical respect; it is surprisingly up-to-date in its coverage of literature; it strives toward, and we believe has essentially succeeded in, "emphasizing ideas and principles rather than factual knowledge." As a highly usable textbook, it is unique in being philosophical, interpretive, skeptical, and opinionated—in a word, stimulating.

The scope of this book is indicated by a list of abbreviated chapter headings: Occurrence and taxonomy of bacteria; General properties; Microscopy; Dyes and staining; Structure; Surface properties; Growth; En-

zymes; Physical factors affecting bacteria; Nutrition; Variation and genetics; Metabolism; and Chemical disinfection. The flow of the textual material is conspicuously uncluttered with references, which occur in sorted selected lists at the end of each chapter. In general, graphic rather than tabular data are used. For a bacteriology textbook, there are relatively few photomicrographs. Large charts of metabolic pathways are included in a back-cover pocket. There are a few editorial lapses that lead to misunderstanding of content.

This book should find its greatest usefulness as a textbook of intermediate complexity for the advanced undergraduate or, better, graduate student who has had some background of biology, biochemistry, physical chemistry, mathematics—and bacteriology. In many departments it will fit admirably into the "advanced general" or "physiological" bacteriology course, as it has for me. For the biochemist, engineer, or other professional who uses bacteria as tools, it should provide an excellent point of general reference. However used, *Basic Bacteriology* should leave the reader with a sharpened taste for the meat of the science.

PHILIPP GERHARDT

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Religion, Philosophy and Psychical Research. Selected essays. C. D. Broad. Harcourt, Brace, New York, 1953. vii + 308 pp. $4.50.

THIS is the second volume of selected essays by C. D. Broad to appear in the series "International Library of Psychology, Philosophy and Scientific Method." Five of the essays are devoted to psychical research, four to religion, and two to politics.

In the first group, one is devoted to Sidgwick and one to Kant and Swedenborg. The opening essay considers the general questions raised by psychical research for philosophy; the second, a more detailed study of telepathy; and the third, the possible application of Dunne's theory of time to precognition.

There are certain basic limiting principles for philosophy that seem to be challenged by the results of psychical research. These principles may be listed under four main heads: causation, limitations on the action of mind on matter, dependence of mind on brain, and limitations on ways of acquiring knowledge. Paranormal cognitions, including precognitions, are accepted as established facts; these would seem to call for a radical change in our conception of time and of causation. In relation to the other principles, it is suggested that consideration should be given to Bergson's theory of normal sense-perception and memory: that the function of the nervous system (including the brain and the sense organs) is mainly eliminative and not productive; they shut out the useless and irrelevant, rather than generate sense data. These considerations are accompanied by a detailed analysis of the factors involved. The chapter on "Normal cognition, clairvoyance and telepathy," originally presented as the presidential address (1935–36) before the Society for Psychical Research, contains a more detailed analysis of some of the problems dealt with in the preceding paper.

Concerning the second section, with its four papers on religion, Broad states in the introduction:

> I have no religious beliefs and, so far as I can judge, I am completely devoid of anything that could fairly be called religious or mystical experience . . . I am concerned simply with the appraisal of *arguments,* which are held by those who use them to be either demonstrative or probable. . . . I have no *anti*-religious emotions or interests.

The first paper considers whether God can properly be called "personal"—that is, "a God which is either a person or is a whole composed of nothing but interrelated persons." The latter is included because Broad insists that the Trinity as such is not a person. A person is a mind that is conscious of its own unity. The word *God* has three different (though related) senses: popular, theological, and philosophic. In the popular sense, God is merely a superhuman person, not necessarily a creator or highly moral. In the theological sense, God need not be a person but must be personal, possesses perfect attributes including moral, is unique and not identical with the universe. In the philosophic sense, God need not be a person. God may be a part on which the rest depends (Deism); or the whole in its unity (Pantheism); or reality (the absolute) as opposed to appearance. Broad is concerned here only with the popular and the theological senses. "What reason, if any, have we to believe in the existence of divine persons?"

Such a belief might appeal to direct knowledge, or to demonstrative proofs, or to authority. None of these seem conclusive for the theological position. But

> . . . these objections do not apply to the notion of Gods in the popular sense. For all I can see there may be dozens of such Gods; and the only reason *against* being a polytheist is that there is no reason *for* being one.

A further essay is devoted to a more extended consideration of the arguments for the existence of God. These arguments are divided into those with premises not ethical or religious; those with premises ethical but not religious; those with religious premises. The classification of the metaphysical arguments is essentially that of Kant. In considering all of these it is necessary to keep in mind the distinction between existential propositions and characterizing propositions, even though in grammatical form they may seem alike. Further, when more clearly expressed, all necessary propositions take the form, "if . . . then"; existential propositions do not. In no case do the conclusions advanced follow.

The third class of arguments appeals to religious experience (that is, mainly, mysticism). The problems that arise here are the psychological analysis of religious experience; its genetics and causal conditions; and its validity, or the justification of its claims to reveal the nature of reality. The conclusion is that at this stage in the development of religious experience it is im-

possible to state how much is delusion and how much is a confused and inadequate expression of some fundamental truths.

The essay "Relations of science and religion" (Christianity in particular) considers two main questions: (i) Do the generally accepted methods and results of the various sciences conflict with Christian doctrines—that is, are they either logically incompatible with those doctrines or such as to render them extremely unlikely to be true? (ii) Do they undermine the only grounds that people have ever had for believing Christian doctrines?

The Christian doctrines may be divided into those *about* Christ and those ethical doctrines taught by Him. A summary of the former is given. For most of these, empirical science has no relevancy. The question of miracles pertains to both.

> If there is conflict, it will be over doctrines like the occurrence of miracles, the immortality of the soul, the freedom of the will, and the question whether moral predicates are significant and applicable to men and their actions. These doctrines are common to all, or nearly all, religions, and they are peculiar to none.

Christianity, however, is markedly geocentric and anthropocentric. And these positions are bound up with theories science can no longer accept. With regard to miracles, a list of restrictive principles, generally accepted, is given, some of which would be violated if miracles actually occurred.

> Psychical research has made it far more probable than not that certain kinds of phenomena which are miraculous, in the sense defined, do in fact occur. . . . With the doubtful exception of psychical research, none of the sciences tells us of anything which lends the least probability to human survival.

In all these essays, it is the detailed analysis that is of value. But to have considered these would have called for another volume.

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Music Therapy. E. Podolsky, Ed. Philosophical Library, New York, 1954. ix + 335 pp. Illus. $4.75.

THE Podolsky-edited *Music Therapy* is a book practically guaranteed to exasperate the serious student of clinical psychology or the psychology of music. The volume's essays range from the extremely trivial to the fairly important, from the frankly anecdotal to the experimentally oriented, from an article on how to treat one's high fidelity tonal equipment to a detailed account of the Lowenstein psychosensory restitution phenomenon. The one chapter in which a really controlled experiment on music therapy is described ends with the note that there was no appreciable carry-over effect of the music after a period of 6 hours. Yet the majority of the other chapter authors assume as self-evident the potency of music as therapeutic material.

Thirty-two already-published articles make up *Music Therapy*'s chapters, but the place of original publication is never given. Instead, a blanket acknowledgment is merely given the 14 journals from which the articles presumably were taken. Hence the researcher must run through the abstracting journals whenever he wishes to learn whether a bit of material is recent or is hopelessly out of date.

Music Therapy will, of course, prove of practical value to the harried therapist who wishes to add another piece of lead to the buckshot with which he now peppers his patients. The musical pharmacopoeia that he will find in the Podolsky book does little violence to common sense. But it does not contain specific remedies, and most certainly, there will be no "satisfaction guaranteed or we will refund your money."

No doubt *Music Therapy* reflects the chaotic condition of the area of which it attempts to give an authoritative picture. At present, music therapy is largely in the hands either of psychiatrists with little experience or interest in research or of music teachers who are abundantly enthusiastic but even more ignorant of experimental methodology. The efforts of these two groups more than likely do considerable good. But how effective they are, no one at present really knows.

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Department of Psychology, Stanford University

Pattern of the Tiger. Stanwell Fletcher. Little, Brown, Boston, 1954. 296 pp. Illus. $5.

STANWELL FLETCHER, an American naturalist, explorer, and lecturer, made a return visit to India and Pakistan during 1951. *Pattern of the Tiger* is an autobiographic account of his travels with his wife and of their impressions and evaluations of their experiences. Taken as a whole, this is an entertaining, often instructive account of the author's observations of persons, places, and customs such as an intelligent American visitor might make during a short trip. But Fletcher attempts much more in this ambitious book and presents us with his evaluation of "India's place in world affairs" and its relationship to Pakistan. He has little sympathy with Hindu India and even less with the Nehru government, but he has much sympathy for the struggling state of Pakistan and unqualified admiration for the virile manhood and charming womanhood of its Moslem population.

Of special interest are the author's description of his visit to the Kashmir region and his account of the Moslem festival of *Muharram* and the Hindu celebration of *Dussehra*. He pays high tribute to the work of the United Nations team in keeping the peace in troubled Kashmir. The highlight of his brief ethnographic descriptions of the peoples of northern India is his dramatic, firsthand account of the remnant of intrepid Moslem Kazakhs who fled before the Russian Communists during World War II and finally found a temporary refuge, first in Tibet and now in Srinagar in northern Kashmir.

The author apparently feels that the Indian govern-

ment has dealt in bad faith in the Kashmir dispute and that India's *de facto* control of the Kashmir region and the Indus River threatens the very existence of West Pakistan. He has few good things to say of Nehru whom he regards as essentially a socialist and Communist-sympathizer. India's alleged neutrality is said to be in fact anti-Western, whereas Pakistan is portrayed as really anti-Communist and the friend of the West. According to the author,

> The desire of Jawaharlal Nehru to bring Pakistan under Hindu domination, while following a course aimed at raising India to the position of leadership of an Asiatic Force in opposition to the West, and to Communism, is not in the best interests of world peace.

Besides being an entertaining and instructive book on travel in India and Pakistan, *Pattern of the Tiger* is a controversial, often dogmatic expression of the author's personal political prejudices which does little to promote the kind of broad cultural and international understanding that is so much needed today. Interesting photographs contribute to the enjoyment of this attractive book.

DAVID BIDNEY
Department of Anthropology, Indiana University

Organic Chemistry. A brief course. Robert Ward Getchell. McGraw-Hill, New York, 1954. vii + 278 pp. $4.

THIS is an eminently usable textbook, obviously written by one who has had extensive experience in teaching and who is aware of the limitations of time and student background which restrict the teacher in the usual service course in organic chemistry. Not an abridgment of a larger textbook, it is clearly designed and written for the students for whom organic chemistry must be a service course or a cultural tool.

In presenting organic chemistry within the space of 278 pages, the author has done a courageous, if not ruthless, job of selecting and pruning. He has made a wise selection of topics, covering essential material and riding no hobbies. Many teachers may wonder, however, whether the almost complete omission of diagrams and illustrations is not, from the pedagogic standpoint, an expensive economy.

Aliphatic and aromatic chemistry are deftly blended and a nice balance between the two is achieved. Integration of the two series permits the author to lead gradually into topics of increasing complexity and to relegate the most difficult chapters to the end of the book.

Except for the emphasis on individual compounds and their practical applications related to the interests of the students for whom the textbook is intended, the treatment is a fairly conventional one. Relatively little attempt is made in its brief space to present fundamental underlying principles. In the infrequent cases in which mechanisms of reactions, such as the haloform reaction and photosynthesis, for example, are treated, rigor is sacrificed for brevity.

Throughout, the writing is clear and concise. Frequently a pleasing informality is achieved. The text is admirably free of typographical errors. Some teachers may object to use of the older term "radical" for the now preferred "group," to the analogy between alcohols and bases and between esters and salts, and to the use of small "d" and "l" to designate configurational families. Occasionally, words are used to describe reactions where equations would be more direct and forceful, and in a few instances, such as in the treatment of optical activity, extended technical discussions are unaccompanied by any clear-cut indication to the student of their importance or significance. But these are relatively minor criticisms and should not obscure the fact that this is a textbook that any teacher who wishes to bring organic chemistry to life for students in the service course should consider seriously.

CALVIN A. VANDERWERF
Department of Chemistry, University of Kansas

Plant Breeding for Everyone. John Y. Beaty. Charles T. Branford, Boston, 1954. 102 pp. Illus. + plates. $2.75.

THE title of this slender volume is at variance with its contents, for the term *plant breeding* implies a genetic basis that is entirely lacking in the major portion of the book. A more precise indication of the author's intentions is found in the subtitle, "How to find and develop new plant varieties." Designed as a practical handbook for amateur gardeners, students, and farmers, it is written in an informal, almost journalistic style that may appeal to the audience for which it is intended but will unfortunately make its information and purpose suspect in the academic mind.

The book opens with staccato accounts of housewives, elderly farmers, and backyard gardeners who have profited financially from the discovery of new plant varieties and states that you, too, can do the same. Nineteen subsequent brief chapters offer pertinent advice on how to recognize, evaluate, and test a new plant variety; how to name, patent, and introduce a new variety; how to evaluate specific characters; how to recognize undesirable characters; and how to keep records. Other topics include hybridization, selection, bud mutations, introduction of varieties from other countries and, finally, how characters are inherited. The field is extensively covered.

The author, a former associate of Luther Burbank, quotes at some length from letters he has received from nurserymen, friends, hybridizers, and others. He refers to experiences of his own and of friends. Burbank's influence is evident throughout in quotations and personal reminiscences.

The need for a practical manual on the development of new plant varieties is admitted, and this volume partially fills the need. It could be debated whether popularization of knowledge requires the manner of a radio commercial.

GORDON W. DILLON
Botanical Museum of Harvard University

Practical Chromatography. R. C. Brimley and F. C. Barrett. Reinhold, New York; Chapman & Hall, London, 1953. 128 pp. Illus. + plates. $5.

ALTHOUGH the first separation of substances by adsorption on filter paper is almost a century old, the first successful application of a chromatic method is generally credited to the Polish botanist Tswett in 1906. Not much progress was made in this technique until the early 1930's. Chromatography has expanded so rapidly during the last 10 years that there is need for a book that will serve as a practical guide to chemists who often require the separation of compounds, frequently present in trace quantities, and sometimes very similar in their structures and properties. *Practical Chromatography* will be an aid not only to analytical and industrial chemists but also to research workers in many fields where chromatographic techniques are used or could be used.

In their treatment of the subject, the authors distinguish three types of chromatography—partition, adsorption, and ion-exchange—and three ways of operating chromatographic columns—elution, frontal analysis, and displacement. Thus, nine chromatographic methods are possible, although some of them have not been investigated. These topics are presented in seven chapters, the last one of which deals with accessory apparatus. The book is adequately illustrated with graphs, diagrams, and photographs. Details of solvents, sprays, and R_F values are given in an appendix, followed by a list of the references (113) cited in the text, and a subject index.

The authors are to be congratulated for a concise, factual introduction to chromatography, with the latest methods and equipment, and many important applications.

Chromatographic Methods of Inorganic Analysis. F. H. Pollard and J. F. W. McOmie. Academic Press, New York; Butterworths, London, 1953. viii + 192 pp. Illus. + plates. $5.50.

THIS book presents an up-to-date introduction to a rapidly growing subject, with special reference to paper chromatography. A brief account is given, in non-mathematical language, of the main theories of chromatography and of those factors that affect inorganic analyses by these methods. Sufficient practical details of the various procedures and apparatus are given so that the beginner can perform the experiments in qualitative and quantitative analysis. The original papers are cited for further details where necessary. Extensive tables are given of the solvent mixtures employed in chromatographic analyses, ". . . including the separation of the groups of metals of the traditional schemes of analysis. In addition, two new schemes of quantitative analysis are described, in which the separation and identification of ions are carried out entirely on filter paper and no previous separation in the classical manner is required." The authors, however, do not suggest that these schemes should replace the older methods, which many regard as of great value in teaching, but rather that they are a very useful supplement to such work, especially when dealing with micro-quantities and when introducing students to new uses for organic spot tests. The book does not include the applications of ion-exchange resins so widely used in chromatographic separations, but it does discuss, in the last chapter, the advantages of combining electromigration with chromatography. Author and subject indexes are included. The paper, printing, and binding are good.

This book will be helpful to chemists and others who are interested in chromatographic inorganic analysis, especially when separations and tests are made by the use of filter paper or columns of cellulose.

JOHN H. YOE
Department of Chemistry, University of Virginia

Man, Rockets and Space. Burr W. Leyson. Dutton, New York, 1954. 188 pp. Illus. $3.50.

CAPTAIN LEYSON is a professional popular science writer whose previous efforts include such titles as *Modern Wonders and How They Work, More Modern Wonders and How They Work, Fighting Crime, Plastics in the World of Tomorrow,* and *Wings around the World*. The eight chapters of the present little book have the following headings: "The origin of our earth and its atmosphere," "The rocket and how it works," "Research rockets," "Cosmic radiation," "Plans for the conquest of space," "Is space travel possible?" "Man in space—conditions during space travel," and "Where to in space?"

In writing the book, the author apparently has thrown together the material he collected from newspapers, Sunday supplements, and other similar sources, without exercising critical judgment on the appropriateness and the accuracy of the material. The result is a book full of mistakes and confusion. Anybody who wishes to have an introduction to the subject of rocket flight in space may well pass this one and read the works of professional fanciers of astronautics, such as W. Ley, A. C. Clarke, and W. von Braun. If nothing else, these people at least have spent a lot of time thinking about the subject and thus are "experts."

H. S. TSIEN
Guggenheim Jet Propulsion Center, California Institute of Technology

Viruses. Cold Spring Harbor Symposia on Quantitative Biology. vol. XVIII. Biological Laboratory, Cold Spring Harbor, N.Y., 1953. xvi + 301 pp. Illus. $8.

VOLUME XVIII of the Cold Spring Harbor Symposia on Quantitative Biology represents a milestone in the field of virus research. Never before in the history of virus research had so many world authorities in the bacterial, animal, and plant virus fields discussed their problems together. Of a total of 41 papers, 20 deal chiefly with bacteriophages, 12 with animal viruses, one

with plant viruses, one with morphology, three with the chemistry of viruses in general, and four with bacteria. The large proportion of papers concerned with bacteriophages demonstrates the great interest and enormous progress made in this topic in the last 10 years. Some methods and results in this field have also influenced strongly the study of animal viruses. Great improvements of tissue-culture techniques will undoubtedly give a very decisive impulse to fundamental and applied research in the whole animal virus field. They also provide new possibilities for attacking certain problems that have looked insoluble until now.

It is not obvious why the plant virus field was represented by only one speaker. Even when considering the sometimes different aims and approaches of plant virology, it would surely have been justified in a colloquium on "viruses," with 41 speakers, to invite more than one plant virologist.

The papers on the chemistry of viruses, unfortunately restricted to nucleic acids, indicate the great importance of chemical analysis for the understanding of virus structures. It is obvious, however, that a lot remains to be done in this field. The inclusion of at least one paper on the morphology of viruses will certainly be appreciated by all biologists. Some results in this field prove to be an efficient complement to the sometimes indirect evidence of biochemical research.

This symposium clearly shows that we are still far away from recognizing general principles valid for all "viruses," chiefly because much more experimental evidence is needed, and partly because viruses are not equally well investigated. However, the symposium has helped enormously in the integration and understanding of the problems of the different virus fields and has stimulated, by give and take, many new ideas. A volume of the Cold Spring Harbor series does not need particular recommendation. It is only regretted that it was impossible to include some of the *esprit* of the actual meeting, unforgettable to the participants.

G. H. Bergold

Forest Insect Pathology Laboratory,
Sault Ste. Marie, Ontario, Canada

Insecticides and Colonial Agricultural Development. Proc. of 6th Symposium of Colston Research Society, 23–27 Mar. 1953. T. Wallace and J. T. Martin, Eds. Butterworths, London, 1954. x + 169 pp. Illus. 30s.

THIS publication consists of a foreword, preface, 16 papers by experts in their respective areas or fields, and discussions of each topic. The topics presented are "The importance of crop protection to the Colonies and its relation to agricultural development," "Some observations on colonial insecticide research," "The control of locusts by insecticides," "La lutte antiacridienne dans les territoires français d'outre-mer," "The control of pests of cotton in tropical Africa," "The design of insecticide field trials on cotton in Uganda," "Insecticide investigations on crops in East Africa," "A problem of insecticides and biological control in East Africa," "Crop protection problems in West Africa," "The role of insecticides in the agricultural development of the British Caribbean territories," "Losses caused by insects in crops during storage and transit," "Insect- and tick-transmitted diseases of livestock," "The control of insects of veterinary importance with special reference to the tsetse fly," "The control of ecto-parasites of livestock," "Spraying technique and machinery," and "Insecticide manufacturers' research and supply problems." In addition to the excellent presentation of these topics, interesting and informative discussions follow each two to three topics.

Each participant in the symposium presents a great wealth of information in an interesting manner. It is the kind of book in which each topic or chapter stands on its own merit. It is gratifying to find that all the major aspects of entomology are recognized by the different participants. Even the health and comfort of the agricultural workers as affected by insects are recognized. Particular stress is given to research, basic and applied. Throughout these proceedings, the great need for further research on the physiology, toxicology, taxonomy, bionomics, and control is stressed. The reader is particularly impressed by the astronomical losses in crops and livestock caused by insects in the British Colonies, especially those in tropical regions.

American entomologists will be greatly impressed by the insect control problems that confront the peasant farmers in the different colonies. This is a grass-roots presentation of economic entomology. Every entomologist and insecticide chemist should have ready access to a copy of this excellent publication, and students of entomology will find this a fascinating presentation of insect control as it affects world production of food and fiber.

Bailey B. Pepper

Department of Entomology, Rutgers University

A History of The College of Pharmacy, Columbia University. Charles W. Ballard. Columbia Univ. Press, New York, 1954. vii + 89 pp. Illus. $2.

THIS volume, issued on the occasion of the bicentennial of the founding of Columbia University, is a well-written account of the first 125 years of the College of Pharmacy. It tells the story of the founding of the College of Pharmacy as the first school of its kind in the state of New York in 1829, as an independent institution, and its present affiliation with Columbia University. It portrays the influence of the college on important legislation for the public welfare (including the food and drug laws and the law requiring college graduation for licensure), and depicts the part played by the college in founding the American Pharmaceutical Association, the New York State Pharmaceutical Association, and the Conference of Pharmaceutical Faculties.

The changes in curriculum required in the transition from an apprenticeship to a scientific system of in-

struction, to meet the needs of a dynamic profession, are set forth. The question of finances, changes in physical location and equipment, faculty, students and alumni, and the manner in which the college became increasingly linked with the resources of Columbia University are explained.

This volume is a significant contribution to the history of pharmaceutical education and to the history of the profession of pharmacy.

GLENN L. JENKINS
School of Pharmacy, Purdue University

The Hand Produced Book. David Diringer. Philosophical Library, New York, 1953. 603 pp. Illus. $15.

THIS account of the long, complex, and one may say exciting way from stick-scratches in the ground to Gutenberg's movable type, if not the last word on the matter, is the most authoritative so far. Gathered in 11 chapters and richly illustrated, Diringer's second great book stands beside his *The Alphabet* as source and resource for all who want to know what is known about the vast subject, and how and where to find the evidences. To one who does not have the author's first book at hand, the frequent references to it, in text and picture, are a deprivation and sometimes a bafflement. But this volume is a step in a long plan toward that ideal whole the great scholars dream of, but would find no joy in attaining. Thus, a preliminary report of a successful decipherment of the Creto-Minoan alphabets, the linear B, soon to be more fully discussed in the *American Journal of Archaeology,* came out after Diringer's census and stock-taking seemed up to date. It seems too that even the Gutenberg date has been set back some centuries, if a recent news story is true by the canon and upon fullest examination. This is the sort of thing the author knows happens, and must happen, and is happy to see, because his book is a compilation of all that is known of what is known. For the many of us who cannot possibly know, he has brought it all together.

Books had a hard time to be books—we can scarcely understand how hard. Ink or whatever ink's ancestor was; paper (and the ancestry of paper is a history in itself); binding and the miracles and disasters of preservation; the long slowness or what seems slowness in finding materials—clay, leather, papyrus, parchment—that would resist man's eternal enemy, damp; the ingenuities and the incredible pains men took to make the written word somehow last—these and the infinite ways of the mind are set forth in this book. "The book follows religion" is the title of the seventh chapter, obviously a central section of the long story of which he tells the earliest and most difficult part. The author locates, describes, and pictures existing clay tablet books, papyrus books, and books on later materials before paper as we know it. He assembles known facts about the preprinting development of the book in the ancient Middle East, central and southern Asia, the Far East, and pre-Columbian America. Nothing that can be said by the amateur—and almost anyone else but Diringer must feel himself so—can express the awe and despair, the humility and reverence, that any well-read man must feel upon reading this great history of man's indomitable effort to preserve his written word. What the rival scholars in the history of the early book may think, I do not know, but it could hardly be other than acknowledgment of tireless endeavor and vast command of discovery, facts, languages, libraries, and the forever-irresistible clues leading to further account.

"Inks, pens, and other writing tools" is an appendix, as handsomely illustrated as the book itself; the plates are most valuable for reference; this is a book for reference—to the red the Egyptians used for letters, to iron-gall from the Middle Ages onward, to the quill, to the hair brush and to much besides. But Diringer says,

> It has hardly been possible within the limits prescribed to do more than stimulate interest by providing sufficient information for the inquiring reader. The latter will probably wish to follow particular developments in greater detail.

He has said it himself, in his preface to the book, in one of those splendidly modest fronts a scholar feels he must for pride put up. Nonsense. What this man knows is enough to fill national libraries. All the confusions of the very present world dim, in the perspective of this book, faithful and long in its testimony to man's hope, man's durability by word.

Emotion is not a scholar's feeling (or is it, after such long labors?) and surely drama, or the sense of it, ought not to allow the postlude, "The fate of books," to seem as moving as it truly is. In a few pages near the end, we feel again the sense of irreparable loss, reading of the havoc wrought by the early leaders of the Reformation upon religious and university libraries. Equally stirring are the instances of rescue and rediscovery. But this full story, although loaded with infinite detail, is a drama of man's enduring desire to be enduringly read. As Diringer says, it is not the contents and literary values that here concern him, but the making of books, the containers; and though temples have fallen and nations disappeared, the books remain, the longed-for immortality.

JOHN HOLMES
Department of English, Tufts College

Industrial Electronics. R. Kretzmann. Philips Technical Library, Eindhoven, Netherlands; Elsevier Press, Houston, Texas, 1953. 236 pp. Illus.

THE book, *Industrial Electronics,* is written with a felicitous style. The author has modestly limited the scope to the operation of electronic components (such as vacuum tubes, thyratrons, and photocells) and to a discussion of certain typical applications (such as timing, counting, motor control, and radio frequency heating). The exposition of the applied art is lucidly descriptive, and mathematics is included with a casual restraint.

As implied in the preface, one of the aims of Kretzmann is to provide plant and production engineers with information about standard electronic techniques, and,

further, to awaken them to creative improvement of present processes by utilization of these techniques. The book should serve this purpose admirably. Furthermore, it is an appropriate introductory textbook for a survey course.

Perhaps the author should have informed the reader that it was assumed in the discussion of automatic devices, that stability criterions are fulfilled. Servo theory, or the general method of automatic control by inverse feedback, is not discussed. Thus, the information is inadequate for the design of automatic equipment. The information is in general insufficient for the design of manually controlled equipment, although certain useful analytical methods are included. At the same time, essential relations are summarized in such a fashion that adequate design description is contained for the reader who is already proficient in electronics.

Taken as a whole, *Industrial Electronics* is a worthwhile contribution to the literature. I am pleased to add it to my technical library.

BEN R. GOSSICK
Department of Physics, Purdue University

Books Reviewed in SCIENCE

3 September

Proceedings of a Conference on the Utilization of Scientific and Professional Manpower (Columbia Univ. Press). Reviewed by Gladys L. Palmer.

Scientific and Technical Papers, Seinen Yokota (Yokota Memorial Committee, Univ. of Tokyo). Reviewed by Tokuo Suita.

Introduction to Aeronautical Dynamics, Manfred Rauscher (Wiley; Chapman & Hall). Reviewed by Hugh L. Dryden.

Sweet Corn, Walter A. Huelsen (Interscience). Reviewed by W. Ralph Singleton.

Methods of Biochemical Analysis, vol I, David Glick, Ed. (Interscience). Reviewed by W. H. Tallent.

Biochemistry and Physiology of Nutrtion, vols. I and II, Geoffrey H. Bourne and George W. Kidder, Eds. (Academic Press). Reviewed by Harry G. Day.

Dvorine Pseudo-Isochromatic Plates, Israel Dvorine (Israel Dvorine). Reviewed by Sidney M. Newhall.

Sea-Birds, James Fisher and R. M. Lockley (Houghton Mifflin). Reviewed by J. T. Nichols.

The Structure of Metals and Alloys, Wilhelm Hume-Rothery and G. V. Raynor (Institute of Metals). Reviewed by Karl Lark-Horovitz.

Linear Operators, Richard G. Cooke (Macmillan; St. Martin's Press). Reviewed by J. T. Schwartz.

Dimensional Methods and Their Applications, C. M. Focken (St. Martin's Press; Edward Arnold). Reviewed by E. C. Creutz.

Optical Instrumentation, George S. Monk and W. H. McCorkle, Eds. (McGraw-Hill). Reviewed by K. W. Meissner.

Scintillation Counters, J. B. Birks (McGraw-Hill). Reviewed by James Schenck.

10 September

Atlas of Exfoliative Cytology, George N. Papanicolaou (Harvard Univ. Press). Reviewed by H. W. Beams.

Physical Meteorology, John C. Johnson (Technology Press, M.I.T.; Wiley; Chapman & Hall). Reviewed by David K. Todd.

Introduction to Chemistry, R. T. Sanderson (Wiley; Chapman & Hall). Reviewed by Wallace R. Brode.

Organic Coating Technology, vol. I, Henry Fleming Payne (Wiley; Chapman & Hall). Reviewed by Martin Salo.

Chromium, A. H. Sully (Academic Press); *Zirconium,* G. L. Miller (Academic Press). Reviewed by E. J. Boyle.

Basic Botany, Fred W. Emerson (Blakiston). Reviewed by Edgar Anderson.

Relaxation Methods, D. N. de G. Allen (McGraw-Hill). Reviewed by Carl H. Walther.

Applied Electronics, Truman S. Gray (Technology Press, M.I.T.; Wiley; Chapman & Hall). Reviewed by James Hillier.

The Proteins, vol. II, pt. A, Hans Neurath and Kenneth Bailey, Eds. (Academic Press). Reviewed by Frank W. Putnam.

Principles of Biochemistry, M. V. Tracey (Pitman). Reviewed by W. E. Cornatzer.

17 September

Introduction to Nuclear Engineering, Raymond L. Murray (Prentice-Hall). Reviewed by Alvin M. Weinberg.

Principles of General Ecology, Angus M. Woodbury (Blakiston). Reviewed by Joel W. Hedgpeth.

Economic Geography, Clarence Fielden Jones and Gordon Gerald Darkenwald (Macmillan). Reviewed by George B. Cressey.

The Origins of Psycho-Analysis, Sigmund Freud (Basic Books). Reviewed by Rudolf Dreikurs.

24 September

Airplane Structures, vol. I, Alfred S. Niles and Joseph S. Newell (Wiley; Chapman & Hall). Reviewed by E. F. Bruhn.

Symbolic Wounds, Bruno Bettelheim (Free Press). Reviewed by George Devereux.

Fluid Dynamics, vol. IV, M. H. Martin, Ed. (McGraw-Hill). Reviewed by Lipman Bers.

Intertidal Invertebrates of the Central California Coast, Ralph I. Smith *et al.* (Univ. of California Press). Reviewed by W. T. Edmondson.

Adaptation in Micro-organisms, R. Davies and E. F. Gale, Eds. (Cambridge Univ. Press). Reviewed by Mark H. Adams.

Introduction to the Chemistry of Enzymes, Keith J. Laidler (McGraw-Hill). Reviewed by Howard B. Bensusan.

The Dynamics of Virus and Rickettsial Infections, Frank W. Hartman, Frank L. Horsfall, Jr., and John G. Kidd, Eds. (Blakiston). Reviewed by Merrill J. Snyder.

A First Course in Ordinary Differential Equations, Rudolph B. Langer (Wiley; Chapman & Hall). Reviewed by M. E. Shanks.

Introduction to Nuclear Engineering, Richard Stephenson (McGraw-Hill). Reviewed by Donald H. Loughridge.

New Books

Aerodynamics. Selected topics in the light of their historical development. Theodore von Karman. Cornell Univ. Press, Ithaca, N.Y., 1954. ix + 203 pp. Illus. $4.75.

Physics of Experimental Method. H. J. J. Braddick. Wiley, New York, 1954. xx + 404 pp. Illus. $7.

Ethics. P. H. Nowell-Smith. Penguin Books, Baltimore, 1954. 324 pp. Paper, $0.85.

Vegetable Tanning Materials. F. N. Howes. Chronica Botanica, Waltham, Mass.; Butterworths, London, 1953. xi + 325 pp. Illus. $5.50.

Economic Geography. Clarence Fielden Jones and Gordon Gerald Darkenwald. Macmillan, New York, rev. ed, 1954. xxv + 612 pp. Illus. $6.75.

The Foundations of Statistics. Leonard J. Savage. Wiley, New York; Chapman & Hall, London, 1954. xv + 294 pp. $6.

Introduction to Psychiatry. O. Spurgeon English and Stuart M. Finch. Norton, New York, 1954. viii + 621 pp. $7.

Dictionary of European History. William S. Roeder, Ed. Philosophical Library, New York, 1954. viii + 316 pp. $6.

Endemic Goiter. The adaptation of man to iodine deficiency. John B. Stanbury *et al.* Harvard Univ. Press, Cambridge, 1954. xii + 209 pp. Illus. $4.

Dreams and Nightmares. J. A. Hadfield. Penguin Books, Baltimore, 1954. xi + 244 pp. Paper, $0.65.

Molecular Theory of Gases and Liquids. Joseph O. Hirschfelder, Charles F. Curtiss, and R. Byron Bird. Wiley, New York; Chapman & Hall, London, 1954. xxvi + 1219 pp. Illus. $20.

The Judgment of History. Marie Collins Swabey. Philosophical Library, New York, 1954. x + 257 pp. $3.75.

Industrial Stoichiometry. Chemical calculations of manufacturing processes. Warren K. Lewis, Arthur H. Radasch, and H. Clay Lewis. McGraw-Hill, New York–London, ed. 2, 1954. xi + 429 pp. Illus. $7.50.

George Davidson: Pioneer West Coast Scientist. Oscar Lewis. Univ. of California Press, Berkeley; Cambridge Univ. Press, London, 1954. viii + 146 pp. + plates. $3.50.

Insect Fact and Folklore. Lucy W. Clausen. Macmillan, New York, 1954. xiv + 194 pp. Illus. $3.50.

A Treatise on Conic Sections. George Salmon. Chelsea Publ., New York, ed. 6, 1954. xv + 399 pp. Illus. Paper, $1.94; cloth, $3.25.

The Structure of Metals and Alloys. William Hume-Rothery and G. V. Raynor. Institute of Metals, London, ed. 3, 1954. viii + 363 pp. Illus. + plates. $5.50.

Learning Theory, Personality Theory, and Clinical Research. The Kentucky Symposium. Donald K. Adams *et al.* Wiley, New York; Chapman & Hall, London, 1954. ix + 164 pp. Illus. $3.50.

Optical Image Evaluation. Proc. of NBS Symposium held 18–20 Oct. 1951. National Bureau of Standards, Washington, 1954 (Order from Supt. of Documents, GPO, Washington 25). x + 289 pp. Illus. $2.25.

The Pharmacologic Principles of Medical Practice. John C. Krantz, Jr., and C. Jelleff Carr. Williams & Wilkins, Baltimore, ed. 3, 1954. xxi + 1183 pp. Illus. + plates. $12.

The Moon Puzzle. A revived classical theory correlating the origin of the moon with many problems in natural science. N. O. Bergquist. Grafisk Forlag, Copenhagen, 1954. xiii + 378 pp.

Microbes and You. Stanley E. Wedberg. Macmillan, New York, 1954. ix + 439 pp. Illus. $4.50.

The Permanent Revolution in Science. Richard L. Schanck. Philosophical Library, New York, 1954. xvi + 112 pp. $3.

Wildcat Strike. Alvin W. Gouldner. Antioch Press, Yellow Springs, Ohio, 1954. 179 pp. Illus. $3.

Physiology and Biochemistry of the Skin. Stephen Rothman. Univ. of Chicago Press, Chicago, 1954. xiii + 741 pp. Illus. $19.50.

Stratigraphy and History of the Moenkopi Formation of Triassic Age. Edwin D. McKee. Geological Society of America, New York, 1954. vii + 133 pp. Illus. + plates.

Pygmies and Dream Giants. Kilton Stewart. Norton, New York, 1954. 295 pp. $3.75.

Teachers Manual for College Chemistry in the Laboratory. No. 2. Lloyd E. Malm and Harper W. Frantz. Freeman, San Francisco, 1954. 341 pp. Illus. Paper, $3.30.

Health and Safety for You. Harold S. Diehl and Anita D. Laton. McGraw-Hill, New York–London, 1954. xii + 515 pp. Illus. $3.76.

Psychoanalysis and the Education of the Child. Gerald H. J. Pearson. Norton, New York, 1954. x + 357 pp. $5.

New and Nonofficial Remedies. Council on Pharmacy and Chemistry, American Medical Association. Lippincott, Philadelphia, 1954. xlix + 609 pp. $2.65.

Fundamentals of Psychoanalytic Technique. Trygve Braatøy. Wiley, New York; Chapman & Hall, London, 1954. xi + 404 pp. $6.

A History of the School of Engineering, Columbia University. James Kip Finch. Columbia Univ. Press, New York, 1954. vii + 138 pp. + plates. $2.75.

Sir Isaac Newton. E. N. da C. Andrade. Collins, London; Macmillian, New York, 1954. 140 pp. Illus. $1.75.

Anatomy of Weeds. Emil Korsmo. Grondahl, Oslo, 1954. 413 pp. Illus. 100 kr.

The Optical Properties of Organic Compounds. Alexander N. Winchell. Academic Press, New York, ed. 2, 1954. xviii + 487 pp. Illus. $12.

Spot Tests. vol. I, *Inorganic Applications* xii + 518 pp. $6.50. vol. II, *Organic Applications.* xv + 436 pp. Illus. $6.25. Fritz Feigl. Trans. by Ralph E. Oesper. Elsevier, Houston–Amsterdam, ed. 4, 1954.

Principles of General Ecology. Angus M. Woodbury. Blakiston, New York, 1954. viii + 503 pp. Illus. $6.

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The Psychiatric Interview. Harry Stack Sullivan. Helen Swick Perry and Mary Ladd Gawel, Eds. Norton, New York, 1954. xxiii + 246 pp. $4.50.

Ships, Machinery, and Mossbacks. The autobiography of a naval engineer. Harold G. Bowen. Princeton Univ. Press, Princeton, N.J., 1954. vii + 397 pp. $6.

Fortschritte der Chemie Organischer Naturstoffe. vol. 10. L. Zechmeister, Ed. Springer, Vienna, 1953. ix + 529 pp. Paper, $19; cloth, $19.80.

The Biochemistry of Clinical Medicine. William S. Hoffman. Year Book Publ., Chicago, 1954. xx + 681 pp. Illus. $12.

Laboratory Manual on Fundamental Principles of Bacteriology. A. J. Salle. McGraw-Hill, New York–London, ed. 4, 1954. x + 176 pp. Illus. + plates. $3.50.

Guide for Safety in the Chemical Laboratory. General Safety Committee of the Manufacturing Chemists' Assoc. Van Nostrand, New York, 1954. xiii + 234 pp. Illus. $4.25.

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American Thought. A critical sketch. Morris R. Cohen. Free Press, Glencoe, Ill., 1954. 360 pp. $5.

World Geography: An Introduction. Loyal Durand, Jr. Holt, New York, 1954. vii + 372 pp. Illus. $5.25.

The Giant Cactus Forest and Its World. A brief biology of the giant cactus forest of our American southwest. Paul Griswold Howes. Duell, Sloan & Pearce, New York; Little, Brown, Boston, 1954. xxv + 258 pp. Illus. + plates. $7.50.

Bilder zur Qualitativen Mikroanalyse Anorganischer Stoffe. Von Wilhelm Geilmann. Verlag Chemie, Weinheim/Bergstr., 1954. 120 pp. Illus. DM. 20.80.

(Continued on page viii)

LETTERS

From "A Lazy Scholar"

The article on "Laziness and the scholarly life" by Leonard Carmichael [*Sci. Monthly* **78**, 208 (1954)] contains a great deal that is true and valuable. For example, most of us will recognize in ourselves his picture of the man who sometimes finds himself so immersed in paper work and routine activities that he imagines he cannot get on with his research. This guilty recognition may have persuaded some young men, inexperienced in the normal life of the research worker, to take other parts of the essay too seriously. There are several escape clauses, especially the last half of the last sentence, but on the whole the thesis seems to be that the true scientist is so immersed in his work that he has no time to wash up for his wife, no time for family life, no time to read newspapers or "half-popular" magazines and no time to enjoy his hobbies.

This is the sort of pompous nonsense that brings science and scientists into disrepute among sensible people. I have been washing up for one woman or another from the age of 7, and I do not believe that it has done my scientific career one scrap of harm. I am not a distinguished scientist, but among my friends are some of the finest scientific intellects on this continent, and they shamelessly sail their boats upon the Ottawa River, not only on weekends, but in the evenings as well! They dig their gardens, build their record-players, paint pictures, and play with their children, just as city clerks do, but on the whole rather more efficiently.

The plain fact is that to be a successful scientist a normal man must live a happy, normal life. To do this, he must be a good citizen and a good husband and father and be glad to enjoy the beauty and order of things outside, as well as within, the limitations of his work. If he is lucky he will experience during his lifetime half a dozen moments of major inspiration, and he is rather more likely to experience these beside a trout stream or in his bath than at his bench. As W. I. B. Beveridge has pointed out in *The Art of Scientific Investigation* (Heinemann, London, 1951), these are the moments upon which a successful career is built and no amount of concentrated thought will conjure them up during the process of meditation.

If any young scientist feels that the dedicated life outlined by Carmichael is not for him (or his family), I would urge him to read the moving words of James C. Braddock, beautifully entitled "R.I.P." [*Science* **117**, 698 (1953)].

C. A. Mawson

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As I tried to suggest in my article, the person who finds that scholarship itself is interesting and rewarding is often one who takes satisfaction in knowledge for its own sake. A persistent, genuine, and emotionally reinforced concern with a problem is not infrequently antecedent to what seems to be its sudden solution.

My critic and I agree, therefore, that dishwashing or "shamelessly sailing boats on the Ottawa River" or any other act is fully justified if it is shown statistically that it is a condition under which worth-while inspirations arise. It is fortunate when those who have these "moments" in association with the watery task of dishwashing or the watery fun of sailing boats or bathing also lead such orderly lives that their inspirations can later be transformed into substantial scientific achievement.

It was my hope in making this address to young scholars to propose in nontechnical language some of the psychological factors that bear upon the academic man's management of his own productive life. In doing this I tried to hint at some possible useful procedures without employing too much of the present-day vocabulary used in the specialized psychology of conation and volition. My hope was that this might be of use to a few of the future academic people who sat before me. All of us have known men of true intellectual promise who have wasted themselves in life for want of a little self-discipline.

I am sorry that this seems to be "pompous nonsense" to my critic. I dread to think what he would have said had I used the words that modern psychology and psychiatry have coined to deal with this area of study. In any case sometime in the future I will try to do better.

Leonard Carmichael

Smithsonian Institution, Washington, D.C.

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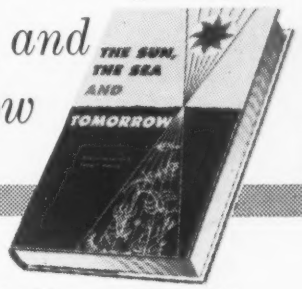

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(*Continued from page 346*)

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General College Chemistry. Andrew J. Scarlett and José Gómez-Ibáñez. Holt, New York, 1954. x + 645 pp. Illus. $6.

Thermo-Mikro-Methoden. Ludwig Kofler and Adelheid Kofler. Verlag Chemie, Weinheim/Bergstr., 1954. xi + 608 pp. Illus. DM. 39.80.

Information Theory in Biology. Henry Quastler, Ed. Univ. of Illinois Press, Urbana, 1953. 273 pp. Illus. Paper, $4.

College Botany. Harry J. Fuller and Oswald Tippo. Holt, New York, rev. ed., 1954. xiv + 993 pp. Illus. $6.90.

Elsevier's Encyclopaedia of Organic Chemistry. Ser. III, Carboisocyclic Compounds; vol. 12B, Naphthalene; sec. A. Compounds containing one naphthalene nucleus. F. Radt, Ed. Elsevier, Amsterdam-Houston, 1953, xlviii + pages 3261–3964. Single issue, $66; series subscriber, $58; complete subscriber, $50.

Reports on Progress in Physics. vol. XVII (1954). A. C. Stickland, Ed. Physical Society, London, 1954. 280 pp. Illus. £2 10s.

Qualitative Analysis and Chemical Equilibrium. T. R. Hogness and Warren C. Johnson. Holt, New York, ed. 4, 1954. xiii + 621 pp. Illus. $5.

Elements of Statistical Mechanics. D. ter Haar. Rinehart, New York, 1954. xix + 468 pp. Illus. $8.50.

Tables Numériques de Physique Nucléaire. Charles Noël Martin. Gauthier-Villars, Paris, 1954. 258 pp. Paper, $5.15; cloth, $6.79.

Mathematical Thinking in the Social Sciences. Paul F. Lazarsfeld, Ed. Free Press, Glencoe, Ill., 1954. 444 pp. $10.

Heat Transmission. William H. McAdams. McGraw-Hill, New York–London, ed. 3, 1954. xiv + 532 pp. Illus. $8.50.

Les Constantes Physiques des Composés Organiques Cristallisés. J. Timmermans. Masson, Paris, 1953. 556 pp. Illus. F. 5200.

Beyond the Germ Theory. The roles of deprivation and stress in health and disease. Iago Galdston, Ed. Health Education Council, New York, 1954. viii + 182 pp. Illus. $4.

Elements of Food Engineering. vol. II, *Unit Operations.* Milton E. Parker. Reinhold, New York, 1954. vi + 360 pp. Illus. $8.50.

Medicine and Science. Lectures to the Laity, No. XVI. Iago Galdston, Ed. International Universities Press, New York, 1954. 159 pp. Illus. $3.

Modern Learning Theory. A critical analysis of five examples. William K. Estes *et al.* Appleton-Century-Crofts, New York, 1954. xv + 379 pp. $5.

Linear Transient Analysis. vol. I, *Lumped-Parameter Two-Terminal Networks.* Ernst Weber. Wiley, New York; Chapman & Hall, London, 1954. xiv + 348 pp. Illus. $7.50.

Handbuch der Pflanzenkrankheiten. vol. II, *Die Virus- und Bakterienkrankheiten,* pt. 1, *Viruskrankheiten.* E. Köhler and M. Klinkowski. O. Appel and H. Richter, Eds. Paul Parey, Berlin, ed. 6, 1954. 784 pp. Illus. DM. 150.

The Anatomy of the Migratory Locust. F. O. Albrecht. Athlone Press, London; John de Graff, New York, 1953. xvi + 118 pp. Illus. $6.

Astrophysics: Nuclear Transformations, Stellar Interiors, and Nebulae. Lawrence H. Aller. Ronald Press, New York, 1954. x + 291 pp. Illus. + plates. $12.

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Evolution as a Process. Julian Huxley, A. C. Hardy, and E. B. Ford, Eds. Macmillan, New York; Allen & Unwin, London, 1954. 367 pp. Illus. $4.25.

Medical Uses of Cortisone. Including hydrocortisone and corticotropin. Francis D. W. Lukens, Ed. Blakiston, New York, 1954. xiii + 534 pp. Illus. $7.50.

Rare Metals Handbook. Clifford A. Hampel, Ed. Reinhold, New York, 1954. xiii + 657 pp. Illus. $12.

Traité de Génétique. vols. I and II. Ph. L'Héritier. Presses Universitaires, Paris, 1954. 518 pp. Illus. Paper: vol. I, F. 1500; vol. II, F. 900.

Student Personnel Work as Deeper Teaching. Esther Lloyd-Jones and Margaret Ruth Smith, Eds. Harper, New York, 1954. xii + 361 pp. $5.

Isotopic Tracers. A theoretical and practical manual for biological students and research workers. G. E. Francis, W. Mulligan, and A. Wormall. Athlone Press, London; John de Graff, New York, 1954. xvi + 306 pp. Illus. $7.

The Struggle for Existence. Umberto D'Ancona. Trans. by Anne Charles and R. F. J. Withers. Brill, Leiden, Netherlands, 1954. xi + 274 pp. Illus.

Irrigated Soils. Their fertility and management. D. W. Thorne and H. B. Peterson. Blakiston, New York, ed. 2, 1954. xii + 392 pp. Illus. $6.50.

Educators Guide to Free Films. Mary Foley Horkheimer and John W. Diffor. Educators Progress Service, Randolph, Wis., ed. 14, 1954. xiv + 566 pp. Paper, $6.

Archaeology in the Field. O. G. S. Crawford. Frederick A. Praeger, New York, 1953. 280 pp. Illus. + plates. $8.50.

Human Physiology. W. B. Youmans. Macmillan, New York, 1954. xiv + 481 pp. Illus. $6.

Theoretical Physics. Mechanics of particles, rigid and elastic bodies, fluids, and heat flow. F. Woodbridge Constant. Addison-Wesley, Cambridge, Mass., 1954. xiv + 281 pp. Illus. $6.50.

Qualitative Inorganic Analysis. A new physico-chemical approach. G. Charlot. Trans. by R. C. Murray. Methuen, London and Wiley, New York, ed. 4, 1954. xi + 354 pp. Illus. $7.

The Symbols of Religious Faith. A preface to an understanding of the nature of religion. Ben Kimpel. Philosophical Library, New York, 1954. x + 198 pp. $3.75.

Weather Modification: Past, Present and Future. Kenneth M. Arenberg *et al.* Weather Modification Group, Wollaston, Mass., 1954. 50 pp. Paper, $3.

Biochemistry and Human Metabolism. Burnham S. Walker, William C. Boyd, and Isaac Asimov. Williams & Wilkins, Baltimore, ed. 2, 1954. xii + 904 pp. Illus. $10.

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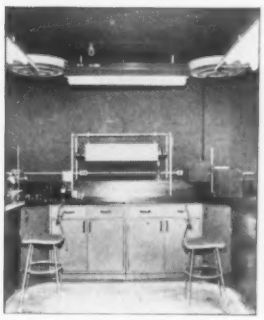

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28–3. American Soc. of Mechanical Engineers, annual, New York City. (O. B. Schier, II, 29 W. 39 St., New York 18.)

29–1. Assoc. of Military Surgeons, Washington, D. C. (R. R. Sayers, Armed Forces Inst. of Pathology, Washington 25.)

29–2. American Medical Assoc., clinical, Miami, Fla. (G. F. Lull, 535 Dearborn St., Chicago 10, Ill.)

December

1–2. Animal Care Panel, Chicago, Ill. (R. J. Flynn, Box 229, Lemont, Ill.)

1–3. American Rocket Soc., New York, N.Y. (ARS, 33 W. 39 St., New York 18.)

1–7. International Cong. on Medicinal and Similar Plants, São Paulo, Brazil. (P. Artigas, Rua Tres Rios 363, São Paulo.)

1–8. Pan American Pharmaceutical and Biochemical Cong., 3rd, São Paulo, Brazil. (C. Fontoura, Rua Caetano Pinto 129, São Paulo.)

3–4. Oklahoma Acad. of Science, Tulsa, Okla. (R. E. Olson, Dept. of Geography, Univ. of Oklahoma, Norman.)

3–5. American Psychoanalytic Assoc., New York, N.Y. (R. L. Frank, 745 5 Ave., New York 22.)

4. American Alpine Club, Philadelphia, Pa. (J. C. Oberlin, 909 Leader Bldg., Cleveland 14, Ohio.)

5. American Acad. of Dental Medicine, 9th mid-annual, New York, N.Y. (W. M. Greenhut, 124 E. 84 St., New York 28.)

5–8. American Soc. of Agricultural Engineers, winter, Chicago, Ill. (F. B. Lanham, ASAE, St. Joseph, Mich.)

5–10. Radiological Soc. of North America, annual, New York, N.Y. (D. S. Childs, 713 E. Genesee St., Syracuse 2, N.Y.)

6–9. Entomological Soc. of America, Houston, Tex. (A. B. Gurney, 1530 P St., NW, Washington 5.)

8–10. Eastern Joint Computer Conf., 4th annual, Philadelphia, Pa. (EJCC, P.O. Box 7825, Philadelphia 1.)

9–11 Texas Acad. of Science, annual, San Antonio, Tex. (G. H. Baird, P.O. Box 228, Huntsville.)

10–12. Florida Acad. of Science, Tallahassee, Fla. (R. A. Edwards, Dept. of Geology, Univ. of Florida, Gainesville.)

11–14. American Acad. of Optometry, annual, Toronto, Canada. (C. C. Koch, 1502 Foshay Tower, Minneapolis 2.)

11–22. World Forestry Cong., 4th, Dehra Dun, India. (T. Street, Foreign Agricultural Service, U.S. Department of Agriculture, Washington 25.)

12–15. American Inst. of Chemical Engineers, annual, New York, N.Y. (AIChE, 120 E. 41 St., New York 17.)

26–28. American Statistical Assoc., Berkeley, Calif. (S. Weiss, 1108 16 St., NW, Washington 6.)

26–29. National Science Teachers Assoc., Berkeley, Calif. (R. H. Carleton, 1201 16 St., NW, Washington 6.)

26–30. Inst. of Mathematical Statistics, Berkeley, Calif. (K. J. Arnold, Dept. of Mathematics, Michigan State College, E. Lansing.)

26–31. American Association for the Advancement of Science, annual, Berkeley, Calif. (R. L. Taylor, 1515 Massachusetts Ave., NW, Washington 5.)

26–31. American Nature Study Soc., Berkeley, Calif. (H. B. Ross, State Teachers College, Fitchburg, Mass.)

27. Metric Assoc., Washington, D.C. (V. G. Shinkle, 1916 Eye St., NW, Washington 6.)

27–28. American Folklore Soc., New York, N.Y. (M. Leach, Bennett Hall, University of Pennsylvania, Philadelphia 4.)

27–28. Ecological Soc. of America, Berkeley, Calif. (J. F. Reed, Dept. of Botany, Univ. of Wyoming, Laramie.)

27–29. American Mathematical Soc., annual, Pittsburgh, Pa. (E. G. Begle, AMS, Yale University, New Haven, Conn.)

27–29. Arctic Inst. of North America, Berkeley, Calif. (R. C. Wallace, 4 Centre St., Kingston, Ont., Canada.)

27–29. Astronomical Soc. of the Pacific, Berkeley, Calif. (S. Einarsson, Leuschner Observatory, Univ. of California, Berkeley 4.)

27–29. International Conf. on Animal Venoms, Berkeley, Calif. (N. Porges, Eastern Regional Research Laboratory at Wyndmoor, Philadelphia, Pa.)

27–29. Western Soc. of Naturalists, Berkeley, Calif. (J. L. Mohr, University of Southern California, Los Angeles 7.)

27–30. Berkeley Symposium on Mathematical Statistics and Probability, 3rd, Berkeley, Calif. (J. Neyman, Dept. of Mathematics, Univ. of California, Berkeley 4.)

27–30. Econometric Soc., Detroit, Mich. (R. L. Cardwell, Cowles Commission, Univ. of Chicago, Chicago 37.)

27–30. National Assoc. of Biology Teachers, Berkeley, Calif. (P. Webster, Bryan City High School, Bryan, Ohio.)

27–30. Soc. of Systematic Zoology, Berkeley, Calif. (R. E. Blackwelder, U.S. National Museum, Washington 25.)

28–29. Linguistic Soc. of America, Detroit, Mich. (A. A. Hill, 1719 Massachusetts Ave., NW, Washington 6.)

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28–30. American Soc. of Zoologists, Chapel Hill, N.C. (R. T. Kempton, Vassar College, Poughkeepsie, N.Y.)

28–30. Archaeological Inst. of America, annual, Boston, Mass. (C. G. Yavis, Andover Hall, Francis Ave., Cambridge 38, Mass.)

28–30. Gerontological Soc., annual, Gainesville, Fla. (N. W. Shock, Baltimore City Hospitals, Baltimore 24, Md.)

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30. Soc. of General Physiologists, Berkeley, Calif. (J. B. Buck, National Institutes of Health, Bethesda 14, Md.)